# AMERICAN ANTIQUITY

ANTHROPOLOGY LIBRARY

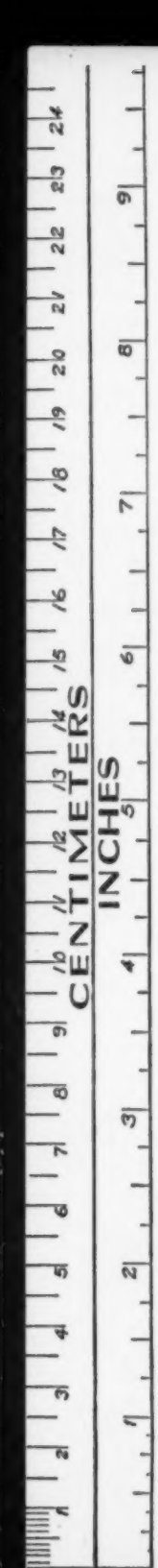
UNIVERSITY
OF MICHIGAN
JAN 27 1954
MUSEUMS
LIBRARY

VOLUME XIX

NUMBER 3

JANUARY 1954

PUBLISHED BY THE SOCIETY
FOR AMERICAN ARCHAEOLOGY




## THE SOCIETY FOR AMERICAN ARCHAEOLOGY

### OFFICERS

*President:* GORDON EKHOLM, Museum of Natural History, Central Park West at 79th Street, New York 24, New York.

*First Vice-President:* GEORGE I. QUIMBY, JR., Chicago Natural History Museum, Chicago 5, Illinois.

*Second Vice-President:* ISABEL KELLY, Niza 53, Zona 6, Mexico, D.F., Mexico.

*Secretary:* JAMES B. GRIFFIN, University Museums Building, University of Michigan, Ann Arbor, Michigan.

*Treasurer:* RICHARD B. WOODBURY, Department of Anthropology, Columbia University, New York 27, New York.

*Editor:* JESSE D. JENNINGS, Department of Anthropology, University of Utah, Salt Lake City 1, Utah.

*Associate Editor:* WILLIAM A. RITCHIE, University of the State of New York, New York State Science Service, State Education Building, Albany 1, New York.

*Assistant Editors:* J. LOUIS GIDDINGS, Arctic; RICHARD D. DAUGHERTY, Pacific Coast; KATHARINE BARTLETT, Southwest; CARLYLE S. SMITH, Plains; DAVID A. BAERREIS, Northern Mississippi Valley; JOHN M. GOGGIN, Southeast; J. ERIC THOMPSON, Middle America; JOHN H. ROWE, South America; and ALEX D. KRIEGER, Early Man; ALFRED K. GUTHE, Northeast.

*Members of the Executive Committee* in addition to the elected officers: FRANK H. H. ROBERTS, JR. (to 1954), Smithsonian Institution, Bureau of American Ethnology, Washington 25, D. C.; ALFRED KIDDER II (to 1954), University Museum, 33rd and Spruce Streets, Philadelphia 4, Pennsylvania; CARL H. CHAPMAN (to 1955), Department of Sociology, University of Missouri, Columbia, Missouri; ERIK K. REED (to 1955), 238 Griffin Street, Santa Fe, New Mexico.

*American Antiquity* is printed by the University of Utah Press, University of Utah, Salt Lake City 1, Utah.

*American Antiquity* is published by the SOCIETY FOR AMERICAN ARCHAEOLOGY at the University of Utah, Salt Lake City 1, during the months of January, April, July, and October. Subscription is by membership. Annual membership dues are $6.00; $5.00 of the dues are for a subscription to *American Antiquity* and other releases, as issued.

Entered as second-class matter June 1, 1951, at the post office at Salt Lake City, Utah, under the Act of March 3, 1879.

**Proposed articles and contributions to "Facts and Comments" should be submitted to the Editor, Jesse D. Jennings; publications for review, reviews, and material for "Notes and News," to the Associate Editor, William A. Ritchie, University of the State of New York, New York State Science Service, State Education Building, Albany 1, New York.**

**Orders for back issues, whole volumes, or memoirs should be sent to Society for American Archaeology, c/o R. S. Peabody Foundation, P.O. Box 71, Andover, Massachusetts. See inside back cover for prices on back numbers.**

**Please address requests for membership or other information to the Acting Secretary, Albert C. Spaulding, at the Museum of Anthropology, University of Michigan, Ann Arbor, Michigan.**

A

Vol.

IN
olog
New
a con
mean
Peruv
conce
Peruv
possib
exist
dle A
nett,

Act
Rinal
South
In ad
gestec
tradit
ern U
Missis

In
to th
South
where
is my
the co
consid
define
and th
demo
co-tra

Ber
tion
co-tra
depth
meant
which
the ar
the fo

1. It
are gre
both re

2. It
regions

AMERICAN ANTIQUITY

VOL. XIX JANUARY, 1954 No. 3

# ON THE USE OF THE CONCEPT OF AREA CO-TRADITION

IRVING ROUSE

IN 1947, at a conference on Peruvian archaeology held in Viking Fund headquarters in New York City, Wendell C. Bennett proposed a concept of area co-tradition and used it as a means of expressing certain characteristics of Peruvian culture history. He first defined the concept in abstract terms, then described the Peruvian co-tradition, and finally suggested the possibility that other area co-traditions might exist in the Southwestern United States, Middle America, and Northwest Argentina (Bennett, 1948).

Acting on Bennett's suggestion, Martin and Rinaldo (1951) subsequently worked out a Southwestern area co-tradition in some detail. In addition, Willey (1953, p. 374) has suggested the existence of an Arctic or Eskimo co-tradition and of three co-traditions in the Eastern United States: Archaic, Woodland, and Mississippi.

In the present paper, I shall confine myself to the use of the concept in Peru and the Southwest, since these are the only places where it has yet been applied in any detail. It is my contention that the authors who applied the concept to the Southwest did not take into consideration all of its features as it had been defined by Bennett and used by him in Peru, and that as a result these authors have failed to demonstrate that there is a Southwestern area co-tradition in Bennett's sense of the term.

## NATURE OF THE CONCEPT

Bennett (1948, p. 1) began his original definition of the concept by saying that an area co-tradition is "a culture area [with] time depth." If I understand this correctly, he meant that it is an ethnographic culture area which can be projected back through time into the archaeology, or in other words that it has the following criteria:

1. Its extension through space and persistence in time are great enough so that it can be divided into series of both regions and periods.

2. Its cultural characteristics occur in all of these regions and periods.

This would seem to be the implication of Bennett's subsequent remark that "an area co-tradition should have certain over-all characteristics which distinguish it as a whole" (*op. cit.*).

But this is not all there is to the concept. Bennett went on in his definition to say that a number of additional features are involved: "Unlike the culture area classification, the cultures included in the area co-tradition are treated as wholes. . . . Each has its own history, its own persistent traditions. The coined word, co-tradition, refers to the linkage, the interrelationships of these cultural traditions in time and space." And again, "the term is not applied merely to the culture history of a region. ...Neither is a co-tradition the history of a single culture.... To repeat, an area co-tradition is the over-all unit of cultural history of an area within which the component cultures have been interrelated over a period of time." Finally, Bennett stated that an area co-tradition "should have definable limits in both space and time. . . . Links between [the various regions where it occurs] and evidences of their being subjected to the same general influences should be found. . . . [Major] changes within the time period covered . . . should be more or less uniform, although not necessarily identical" (Bennett, 1948, pp. 1-2).[1]

If I understand these remarks correctly, they indicate that an area co-tradition is to be considered not only a culture area with time depth but also a grouping of cultures with certain very definite characteristics, as follows:

1. Taken together, these cultures are to be found in all of the regions into which the culture area has been divided during each of the periods when it was in existence.

2. The cultures which succeeded each other in time are linked together by a series of traditions. Bennett does not characterize the traditions, but I assume that he had in mind the standard conception in Peruvian

[1] In assembling the above quotation, I have changed the order somewhat.

archaeology of a series of traits which have tended to persist through a sequence of cultures within a single region (Willey, 1945, pp. 53-5).

3. Some of the cultures which were contemporaneous are similarly linked together through space by a series of traits which tended to diffuse from one culture to the rest. Again, Bennett does not characterize these traits, but I assume that he had in mind the so-called horizon styles which spread through the various regional subdivisions of the Peruvian culture area during certain periods in its existence (Willey, 1945, pp. 49-50).

4. Finally, when the various cultures comprising the co-tradition are laid out according to their distribution in time and space, they reveal the same general stages of development appearing at approximately the same time in all of the regions where the co-tradition occurs.

An area co-tradition, then, is both a culture area with time depth and a group of linked cultures, and the two of them must coincide in distribution. The use of the terms "area" and "co-tradition" for the concept symbolizes this duality.

## Application of the Concept to Peru

That the above interpretation of Bennett's remarks is correct is shown by the manner in which he went about defining the Peruvian co-tradition in the second part of his article (Bennett, 1948, pp. 2-6). First, he showed that there is a culture area with time depth in Peru by carefully defining its limits in both space and time and by listing its cultural characteristics. He noted that "these characteristics are found in each subdivision and each main time period of the Peruvian [culture area]" (p. 3).

Bennett then went on to discuss the linkages between the cultures in the area: "Specific traditions link the cultures of some subdivisions throughout several time periods. Various types of horizon styles cut across some or all of the subregions in one time period. . . . Finally, the culture changes throughout the Peruvian area are roughly uniform in time and direction" (p. 3). He illustrated these points by a detailed discussion of the regional and temporal divisions of the Peruvian culture area, during the course of which he presented a map of the regions and referred to his then unpublished chart of the temporal divisions (Bennett, 1948, Fig. 2; Bennett and Bird, 1949, Fig. 19).

If I may use a simile, the picture I obtain from this is one of a rectangular piece of cloth, the side edges of which represent the geographical limits of both the Peruvian culture area and the Peruvian co-tradition, and the bottom and top edges, the limits of these two in time. The warp threads of the cloth consist of a series of regional traditions running from the bottom towards the top of the cloth, while the weft is composed of a number of horizon styles which extend from one side of the cloth towards the other. The cloth is decorated with a series of irregularly arranged rectangles, each representing a single culture, and these are so colored that they appear to form a series of horizontal bands, representing the various stages in the development of the co-tradition.

## Application of the Concept to the Southwest

In applying the concept of area co-tradition to the Southwest, Martin and Rinaldo (1951, pp. 215-16) begin in the same way as Bennett by pointing to the existence of a culture area with time depth. They define its limits in space and time (p. 218) and list its characteristics (pp. 216-17) as Bennett did. It is noteworthy, however, that these characteristics are not all found in each main time period and in each regional division, as they are in Peru. On the contrary, a number of the characteristics are limited to the earlier or later periods and others are said to be alternatives, which means, I presume, that they occurred in separate parts of the area. To this extent, Martin and Rinaldo's Southwestern culture area differs from the kind of area defined by Bennett and from the Peruvian culture area as he described it.

Martin and Rinaldo's grouping of cultures into a Southwestern co-tradition also differs from Bennett's concept of a co-tradition and from his application of that concept to Peru in several important respects. They name three major "sub-cultures" in the area: Hohokam, Mogollon, and Anasazi (p. 216). These, however, are not strictly comparable to the Peruvian cultures in that they consist of a series of stages extending throughout the period of the co-tradition (Fig. 1). To translate these into Bennett's terms, it will be necessary to call the individual stages "cultures" and to refer to Hohokam, Mogollon, and Anasazi as "sequences of cultures."

Let us examine the "cultures" from the standpoint of the five criteria we have listed for a co-tradition in discussing Bennett's definition of the concept. The first is that the cultures comprising the co-tradition should occur in all of the regional subdivisions of the culture area during all of its periods of existence. This

is difficult to determine from Martin and Rinaldo's article, since they do not give the geographical distribution of their individual cultures. They do say, however, that the Navaho and Apache are "not included because [their] history is not an integral part of the cultural continuum of the area" (p. 213), and from this it may be inferred that the co-tradition does not, in fact, occur in all regions during all periods.

The second criterion of the concept, as Bennett defined and used it, is that the cultures should be linked through time by a series of regional traditions. Here, we are again faced with a lack of information. It may well be that the cultures in the Hohokam, Mogollon, and Anasazi sequences each share a series of traits in the Peruvian fashion, but these traits are not given by Martin and Rinaldo. Neither are we told whether such traits were limited to individual regions, as they were in Peru. We may assume that they were not, however, since the three sequences of cultures tended to spread from one region to another. For example, as Martin and Rinaldo imply (Fig. 1), the Anasazi abandoned parts of their original region after 1000 A.D. and moved into the regions previously occupied by the Hohokam and Mogollon peoples, in contrast to the situation in Peru.

As for trait linkages through space, Martin and Rinaldo (1951, pp. 217-18) point only to a series of traits "that are common to the whole area" (and which might therefore perhaps better be considered characteristics of the culture area) and to the results of trade. No mention is made of any culture traits, as opposed to trade objects, having spread throughout the area within a single period, in the manner of the Peruvian horizon styles.

The final characteristic of the co-tradition, as Bennett defined the concept and used it in Peru, is that the major cultural changes from period to period should be more or less uniform in all parts of the area. Martin and Rinaldo (1951, pp. 218-27) devote the major part of their article to this point, but again fail to show by means of a study of geographical distributions that it was true for all of the regional subdivisions of the Southwestern culture area, as it was in the case of Peru. Certainly, it was not true during the latest periods, when so much of the area was occupied by tribes like the Navaho and Apache which did not belong to the co-tradition and therefore could not have participated in the general trends of cultural development.

It would seem, then, that Martin and Rinaldo have not proved the existence in the Southwest of either a culture area or a co-tradition corresponding to the Peruvian area co-tradition. They fail to show that the characteristics of the Southwestern culture area are distributed throughout the area as they are in Peru. They have not attempted to demonstrate that there is a Southwestern co-tradition with cultures linked together in the Peruvian manner; nor do they find that the distribution of the co-tradition coincides with that of the culture area, as it does in Peru.

To return to the simile of cloth, the situation in the Southwest calls to mind an irregular piece, the jagged edges of which symbolize the fact that the co-tradition does not have the same distribution as the culture area. This cloth is composed of gauze, in which a regular series of warp and weft threads, symbolizing the regional traditions and horizon styles which occur in Peru, are replaced by loosely twisted threads in an irregular arrangement. The designs on this cloth are also irregular and it has a number of gaps, representing cultures like the Navaho which do not belong to the co-tradition but nevertheless have penetrated to the center of the culture area.

## APPLICABILITY OF THE CONCEPT

If these differences between the culture history of Peru and the Southwest are correct, the question arises why they were overlooked by Bennett, when he tentatively suggested applying the concept of co-tradition to the Southwest, and by Martin and Rinaldo, when they worked out their Southwestern co-tradition. In my opinion, the main reason is that, whereas Peruvian archaeologists are accustomed to trace the distribution of culture in terms of both periods and regions, Southwestern archaeologists tend to trace it only according to periods and thereby to obscure the regional variations which are such an important subject of study in Peru.[2] This may be seen by comparing Ben-

[2] This is not to say that Southwestern archaeologists have not recognized regional distinctions in culture, for they have done so in a number of cases (e.g., Reed, 1951). However, they have not formulated a well-defined and generally accepted series of regional divisions comparable to the divisions of time in the Pecos classification, for example, nor have they combined regional and temporal divisions in the manner of Bennett's chart (Bennett and Bird, 1949, Fig. 19).

nett's chart of the distribution of cultures in Peru (in Bennett and Bird, 1949, Fig. 19) with the chart published by Martin and Rinaldo (1951, Fig. 1). In both, the vertical dimension indicates periods of time but only in Bennett's chart is the horizontal dimension correspondingly divided into a series of regions. In Martin and Rinaldo's chart, the horizontal dimension is instead marked off according to the Hohokam, Mogollon, and Anasazi sequences which, as already noted, appear to have moved back and forth from one region to another. Even in their text, Martin and Rinaldo do not divide the Southwestern culture area into regions, whereas Bennett (1948) devoted his only illustration (Fig. 2) and a major part of his text to the corresponding Peruvian divisions.

I would suggest that Bennett, and Martin and Rinaldo, have been misled by the lack of well-defined regional divisions in the Southwest to assume a greater degree of uniformity in the geographical distribution of culture than actually exists there. The establishment of a comprehensive series of regional divisions is needed in the Southwest, so that the distribution of both traits and cultures can be traced from region to region as well as from period to period, in the Peruvian manner. If and when this is done, it should, in my opinion, confirm the differences in the culture history of the two areas which have been indicated above.

In view of these differences, I do not believe that the concept of area co-tradition, as Bennett defined it, is applicable to the Southwest. It would be possible to redefine the concept so as to fit the Southwest, but in my opinion this would be a mistake, since it would destroy the concept's utility as an indicator of the kind of culture change which has occurred in Peru. This kind of change may not be as widespread as Bennett originally suggested, but nevertheless I suspect that detailed studies of both the spatial and temporal distribution of culture would reveal its existence in several other parts of the New World, and possibly of the Old World as well; e.g., in Mesoamerica and Northwest Argentina, as Bennett (1949, p. 7) has suggested. It seems to me advisable to retain the concept in its present form as a means of identifying the Peruvian kind of culture change wherever it may be found.

Elsewhere, there would appear to be no reason why the concepts of culture area and co-tradition should not be used separately as the occasion demands. In the Southwest, for example, I believe it would be possible to define a culture area having both regional and temporal divisions like those in Peru, although it might be necessary to exclude the preceramic period and the time after the Apache and Navaho reached the central part of the Southwest. I doubt, however, that it would also be possible to demonstrate the existence of a Southwestern co-tradition. I am inclined to regard Anasazi and Hohokam, at least, as separate cultures, having their own histories; and if this is correct, they cannot be considered a co-tradition since Bennett, as noted above, specifically excludes the histories of single cultures.[2]

By contrast, a culture area cannot be set up in the West Indies, because the preceramic Ciboney and ceramic "Arawak" Indians have lived together there for some time. Nevertheless, I believe that the "Arawak" cultures can be considered a co-tradition, for a series of regional traditions have been identified and these are linked together by horizon styles (Rouse, 1951).

Finally, as Bennett (1941, p. 8) has pointed out, many parts of the world lack both culture areas and co-traditions. All this, however, does not seem to me to detract from the utility of the concept as an indicator of the specific kind of culture change which occurred in Peru.

## Conclusions

When Bennett (1948, p. 2) originally proposed the concept of area co-tradition, he stated that one of his purposes was to provide a mechanism for comparing the culture history of two or more regions in order to bring out similarities and differences in their manner of culture change. He further suggested that when archaeologists encounter such differences, they should seek their causes.

Accordingly, it is pertinent to inquire why the differences between Peruvian and Southwestern culture history which have been brought out above came into existence. The principal reason, I would suggest, is that the Peruvian Indians tended to remain in the regions they had originally settled, whereas the Southwestern In-

[2] It might be argued that Salado culture represents a link between Anasazi and Hohokam of sufficient strength to consider the two a co-tradition. To me, however, it indicates the opposite, since I am impressed with the manner in which the Salado people appear to have retained their cultural identity when they moved into Hohokam territory after 1000 A.D.

dians often moved from one region to another. In Peru, as a result, the diffusion of culture seems to have taken place primarily by the spread of culture traits from one local group of people to another — whether through trade, conquest, or some other mechanism — but in the Southwest it was apparently more common for cultures to be carried *in toto* by migrating groups of Indians.[4] This in turn may be partially, at least, due to the fact that the Peruvian Indians had developed a more sedentary form of agriculture than most Southwesterners (the Hohokam excepted).

In addition, I suspect that the differences may reflect a greater receptivity to foreign ideas on the part of the Peruvian Indians and a stronger feeling for the integrity of one's own culture in the Southwest — as is indicated by the more ready acceptance of Christianity in Peru than among the Southwestern Indians. Back of this may have been the fact that, even in prehistoric time, the Peruvian cultures were part of a major civilization and therefore folk cultures in Foster's (1953, pp. 170-1) use of the term, whereas the Southwestern cultures were independent entities, not subject to influences on a regional or national scale.

Steward (1948 and 1949) has recently emphasized the need for seeking out similarities in the manner of cultural development among the more advanced peoples of the world. Following Bennett and Willey (1950), I would suggest that it is also important to look for and to explain differences in the manner of culture change. As I have tried to illustrate above, Bennett's concept of area co-tradition is a useful tool for this purpose, rigid and specific though it may be.

[4] This is not meant to imply that trade was unimportant in the Southwest, for it has been widespread (e.g., Colton, 1941). However, this trade apparently did not result in so much diffusion of culture traits from one local group to another as in Peru. What spread seems to have been primarily the articles of trade rather than the ideas or customs behind them.

## BIBLIOGRAPHY


BENNETT, WENDELL C.

1948. The Peruvian Co-Tradition. In "A Reappraisal of Peruvian Archaeology," *Memoirs of the Society for American Archaeology*, No. 4, pp. 1-7.

BENNETT, WENDELL C. AND JUNIUS B. BIRD

1949. Andean Culture History. *Handbook Series, American Museum of Natural History*, No. 15.

COLTON, H. S.

1941. Prehistoric Trade in the Southwest. *Scientific Monthly*, Vol. 52, No. 4, pp. 308-19.

FOSTER, GEORGE M.

1953. What is Folk Culture? *American Anthropologist*, n.s., Vol. 55, No. 2, pp. 159-73.

MARTIN, PAUL S. AND JOHN B. RINALDO

1951. The Southwestern Co-Tradition. *Southwestern Journal of Anthropology*, Vol. 7, No. 3, pp. 215-29.

REED, ERIK K.

1951. Cultural Areas of the Pre-Spanish Southwest. *New Mexico Quarterly*, Vol. 21, No. 4, pp. 428-39.

ROUSE, IRVING

1951. Areas and Periods of Culture in the Greater Antilles. *Southwestern Journal of Anthropology*, Vol. 7, No. 3, pp. 248-65.

STEWARD, JULIAN H.

1948. A Functional-Developmental Classification of American High Cultures. In "A Reappraisal of Peruvian Archaeology," *Memoirs of the Society for American Archaeology*, No. 4, pp. 103-4.

1949. Cultural Causality and Law: a Trial Formulation of the Development of Early Civilizations. *American Anthropologist*, Vol. 51, No. 1, pp. 1-27.

WILLEY, GORDON R.

1945. Horizon Styles and Pottery Traditions in Peruvian Archaeology. *American Antiquity*, Vol. 11, No. 1, pp. 49-56.

1950. Growth Trends in New World Cultures. In *For the Dean: Essays in Anthropology in Honor of Byron Cummings*, pp. 223-47. Santa Fe.

1953. Archeological Theories and Interpretation: New World. In *Anthropology Today, an Encyclopedic Inventory*, pp. 361-85.



Department of Anthropology
Yale University
New Haven, Connecticut

May, 1953

# SUICIDE, SACRIFICE AND MUTILATIONS IN BURIALS AT VENADO BEACH, PANAMA

S. K. Lothrop

VENADO BEACH comprises a shore line facing the Pacific Ocean in the southwestern corner of the Panama Canal zone. The burials here discussed were found 100 to 200 yards from the high water mark in a band of refuse which extended across the mouth of the Mangopobre Valley. A total of 369 bodies have been recorded. Of these, 202 were excavated by the Peabody Museum of Harvard University in 1951, and 167 were unearthed by Neville A. Harte, who most generously has placed at my disposal his carefully kept field notes.

The skeletal material from Venado Beach, owing to unusual soil conditions, is better preserved than any other collection yet found in the wet tropics of the New World. When studied, it should prove of general interest. We are now concerned, however, with burial techniques rather than with the bones themselves or the funeral paraphernalia which accompanied them.

Venado Beach today is part of the target area of Fort Kobbe, a military post extending several miles inland. Archaeological remains, however, were discovered by the U.S. Navy in 1948 as a result of bulldozing off the topsoil with which to enrich the lawns of the 15th Naval District. Any surface markers of graves have therefore been destroyed and the original depths must remain a matter of conjecture. Some graves are over 10 feet below the present surface and the original depth may have been as much as 15 feet.

Many of the recently found burials were discovered in or just under the still-undisturbed black topsoil and were 3 to 5 feet below the present surface. This black layer rested on old raised beaches of white sand or on coquina, vast shell lenses as much as 7 feet thick which had turned to stone. A few water-worn sherds occurred in and under the coquina, indicating a substantial archaeological age, but a comparatively recent geological origin. The complicated chronological and geological problems involved in changing beach levels will not be discussed at present.

Floors of the deeper graves usually were on or in the coquina (Fig. 60). Often a trough was cut in which a body lay, either flexed on the back or side or in an extended position face up, which was locally known as a "bathtub burial" (Fig. 61, *b*).* When the coquina was sufficiently thick and the individual sufficiently important to command the labor, globular chambers with a step or bench on the side were cut in the rock (Fig. 60). These sometimes contained several bodies.

Venado Beach graves might be individual and scattered, or they might occur in compact groups with as many as fifty bodies. Both individual and grouped graves often were blanketed by a layer of red clay (Fig. 61, *a*) which must have been secured on the adjacent hills.

At the Sitio Conte in the Province of Coclé, where large graves also have been found, the principal occupant can be distinguished by his central position and abundant jewelry. This is not usually true at Venado Beach, but the relationship of graves in a given group perhaps can be determined when the funeral furnishings are studied.

Venado Beach is only about 10 miles distant by water from the sixteenth century Spanish settlement now known as Old Panama. Among the first officials who lived there was Gonzalo Fernández de Oviedo, who later became Royal Historian. He must have known natives very similar to those at Venado Beach if indeed he did not visit this settlement himself.

Oviedo has described burial customs of the Isthmus in great detail. In general, it seems that funeral ceremonies were regarded as magical rites which gave access to another world, primarily reserved for the chiefs and the nobility. Commoners and slaves normally were not buried at all, and their bodies were consumed by birds and beasts. Certain individuals from the lower classes, however, might be selected by their betters for service in a future life. To this end, they either killed themselves or were slaughtered, and their bodies were placed in the graves of their masters.

* The photographs used in Figures 61 and 63 have been slightly modified by retouching. Retouch was confined to outlining edges of objects or bones to enhance contrast.

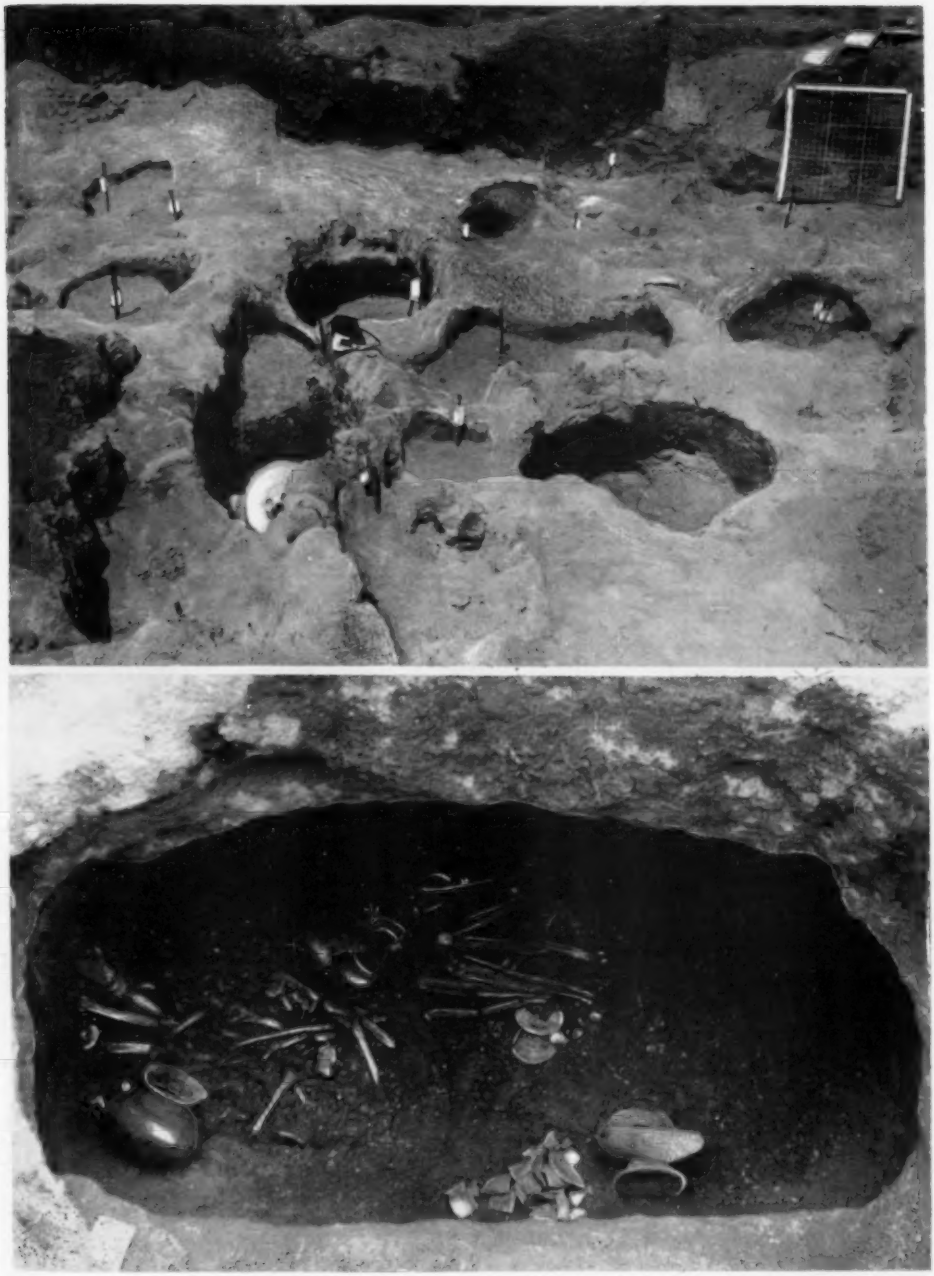


FIG. 60. *Top*. Graves cut in coquina. *Bottom*. Coquina-cut grave containing two disarticulated bodies. Venado Beach, Panama.

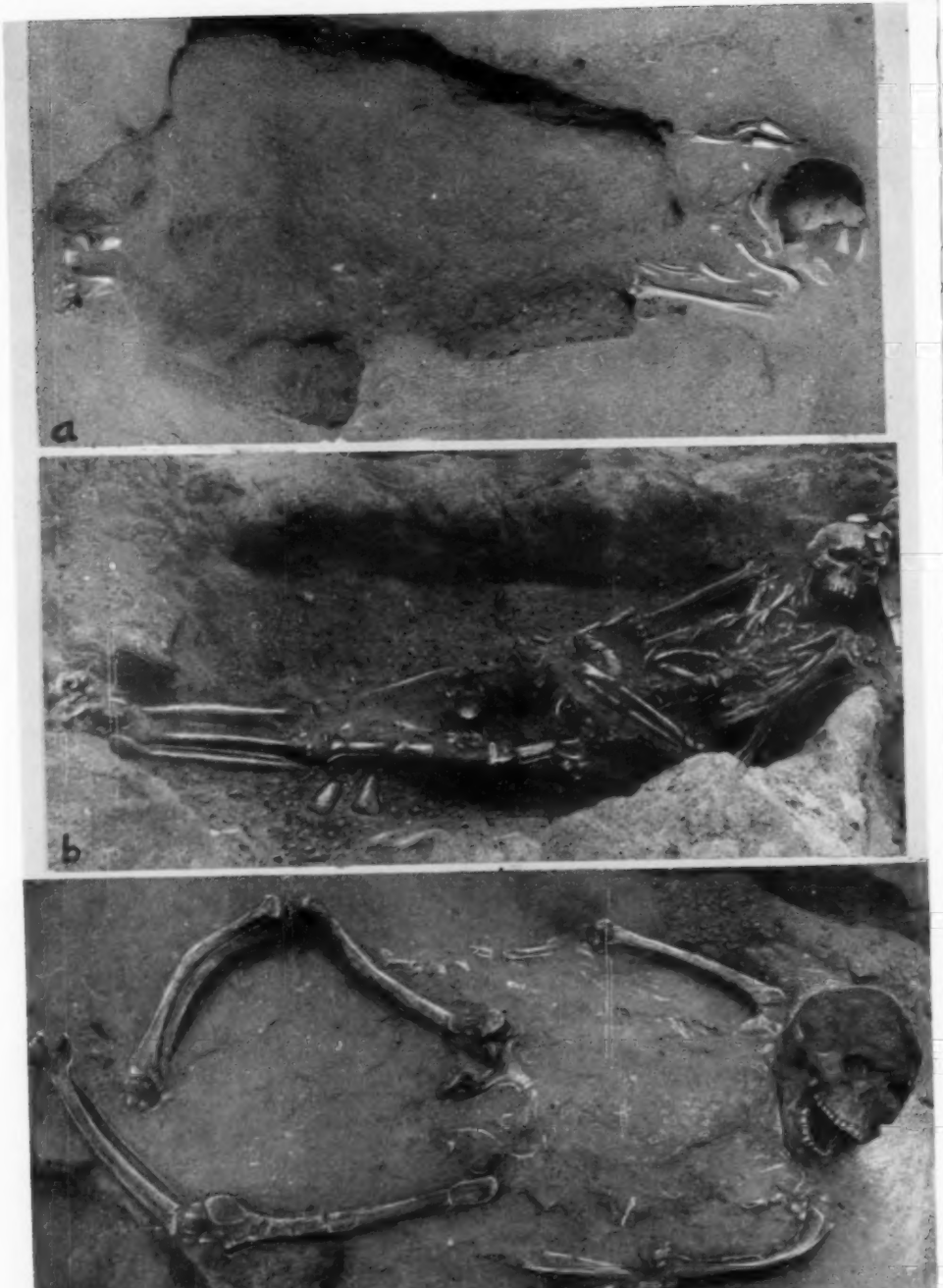



FIG. 61. Extended burials. *a*, body covered by a clay blanket; *b*, body in a trench cut in coquina; *c*, body of a dwarf on flat coquina. Open mouths in *b* and *c* may indicate burial alive.

*Suicide.* Oviedo implies that, when a commoner accompanied a nobleman to the next world, it was done on a voluntary basis. Either the individual took poison or was buried alive while stupefied by alcohol. This choice between immediate death with the promise of a future life or ultimate oblivion must have been a hard one. Usually only beautiful women and stalwart young men were selected for the journey. Of fifty-one bodies observed in five large Sitio Conte graves in Coclé, only six had closed sutures and could be classified as old — apart from some of the principal occupants.

We cannot expect to find osteological evidence of suicide, but, in Coclé, where bodies were placed in ordered rows like logs of wood, we presume death had taken place before interment. Similar evidence by position was found by Neville Harte at Venado Beach in a burial with fifteen flexed bodies placed in orderly rows.

In regard to burial while intoxicated, we found indications that certain individuals at Venado Beach had been buried alive. Some were found with mouths open as if gasping for air (Fig. 61, *b*, *c*), and there were a few people who died while biting their fingers — which suggests movement of the hands after inhumation.

*Sacrifice.* It seems probable that those who suffered an involuntary death were slaves, usually prisoners of war, whose lot was a hard one. Oviedo states that the property mark of their owner often was tattooed on their faces and that front teeth might be knocked out. Thus permanently disfigured, they would have little inclination to escape to their own people.

One method of killing at Venado Beach was by breaking the back just above the lumbar vertebrae or by breaking the neck. Evidence of both was positive, for the bodies rested on flat coquina where lateral displacement of the bones was impossible. In two instances, individuals with broken backs had been placed in the ground face up with the knees flexed beside one shoulder. In their arms they held a bundle of bones representing their masters (Fig. 62, *a*). The flesh had been removed and, in at least one case, the bones had been painted purple. Broken necks were usually detected at once owing to abnormal position of the head (Fig. 63, *c*). In one instance we were able to harden the entire neck in the ground and remove it as a unit. It had received such a blow that the spines pointed in different directions (Fig. 63, *b*).

Many burials in urns were found at Venado Beach. Sometimes they contained bundle burials of adults. Usually, however, they held the articulated bodies of infants, and the urn had been placed beside an adult body (Fig. 63, *a*). The presumption is that these children were expected to grow up and serve in another world. Sometimes the relationship was reversed. For instance, a rich child had been placed in an urn with gold jewelry and with elaborate pottery both inside and surrounding the urn. Nearby lay an adult body, presumably of a servant, with no funeral furnishings (Fig. 64, *b*).

*Mutilations.* Nearly 25% of the bodies at Venado Beach had been mutilated in two principal ways, which we may classify as bundle burials and ritual burials.

The bundle burials consisted of bones which had been preserved after stripping off the flesh. They evidently represented important individuals as attested by sacrificed slaves found with them. Funeral paraphernalia tended to be more elaborate than in other graves. One burial was outstanding. In a large globular tomb cut in hard coquina we found a foot, three cervical vertebrae and a few ribs. The rest of the disarticulated body was in an even larger adjacent coquina-cut grave, and a score of other bodies lay nearby (Fig. 60). This group must have represented a very important chief.

The ritual mutilations are of several types (Table 8), the simplest being decapitation. Sometimes the severed head was buried with the body; sometimes it was removed, presumably to serve as a trophy. Also, separate heads which might have been trophies were found with complete bodies. We observed a very curious mutilation in four graves. In each case, the cranium had been removed, leaving the neck and lower jaw intact and attached to the body. A count of the teeth in the lower jaw showed that all were present but, inside or along side the jaw there were two extra canine teeth, presumably extracted from the upper jaw (Fig. 64, *a*). The operation had been carried out with a skill and precision suggesting long practice, for we noted that neither the condyles of the jaw nor the neck bones had been marked in the cutting. In several cases we found com-

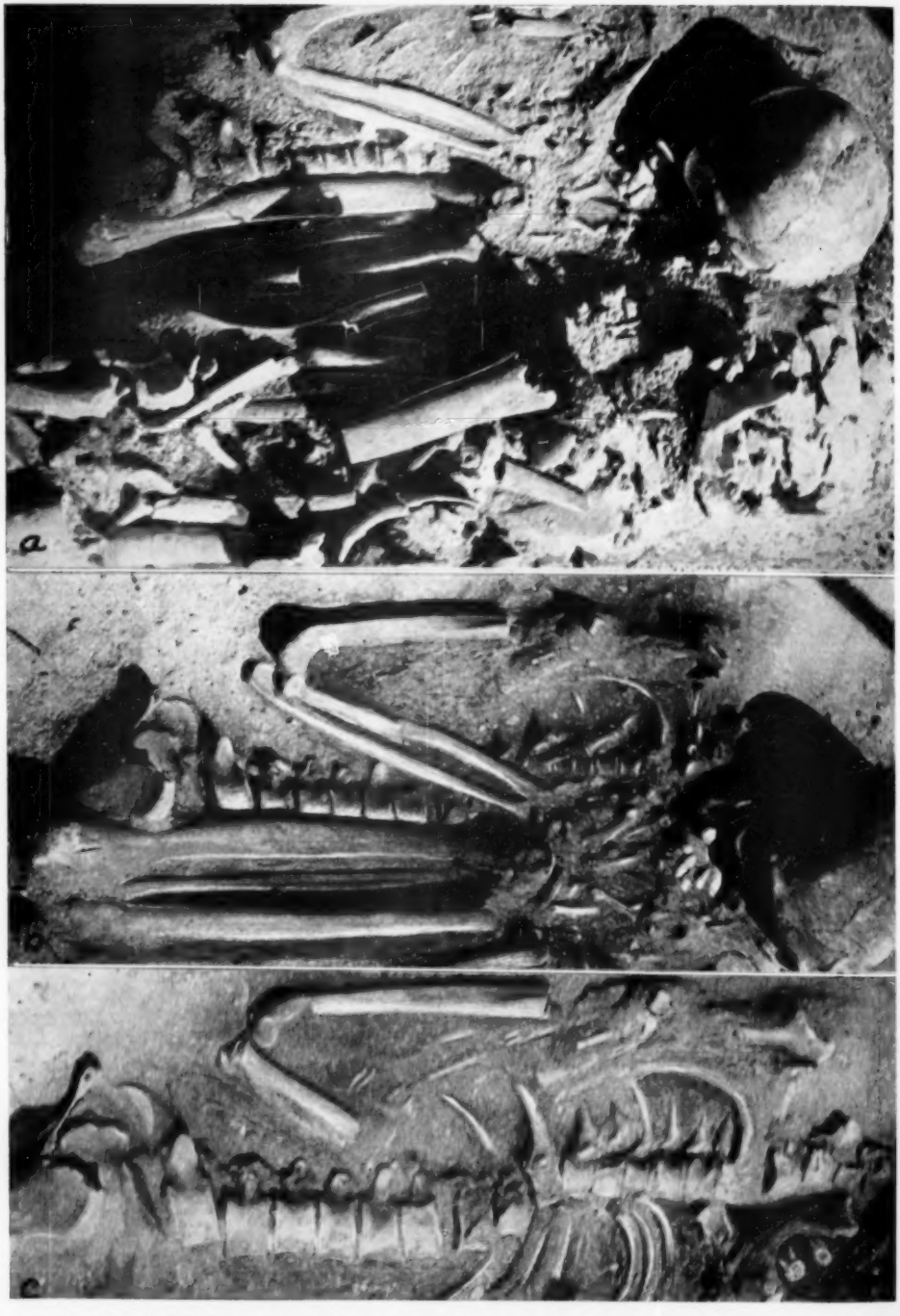



FIG. 62. A bundle burial over a flexed burial with a broken back. The bundle burial fills the lower half of *a*. In *b* it has been removed. In *c* the forearm of the flexed burial has been removed to show the broken back.

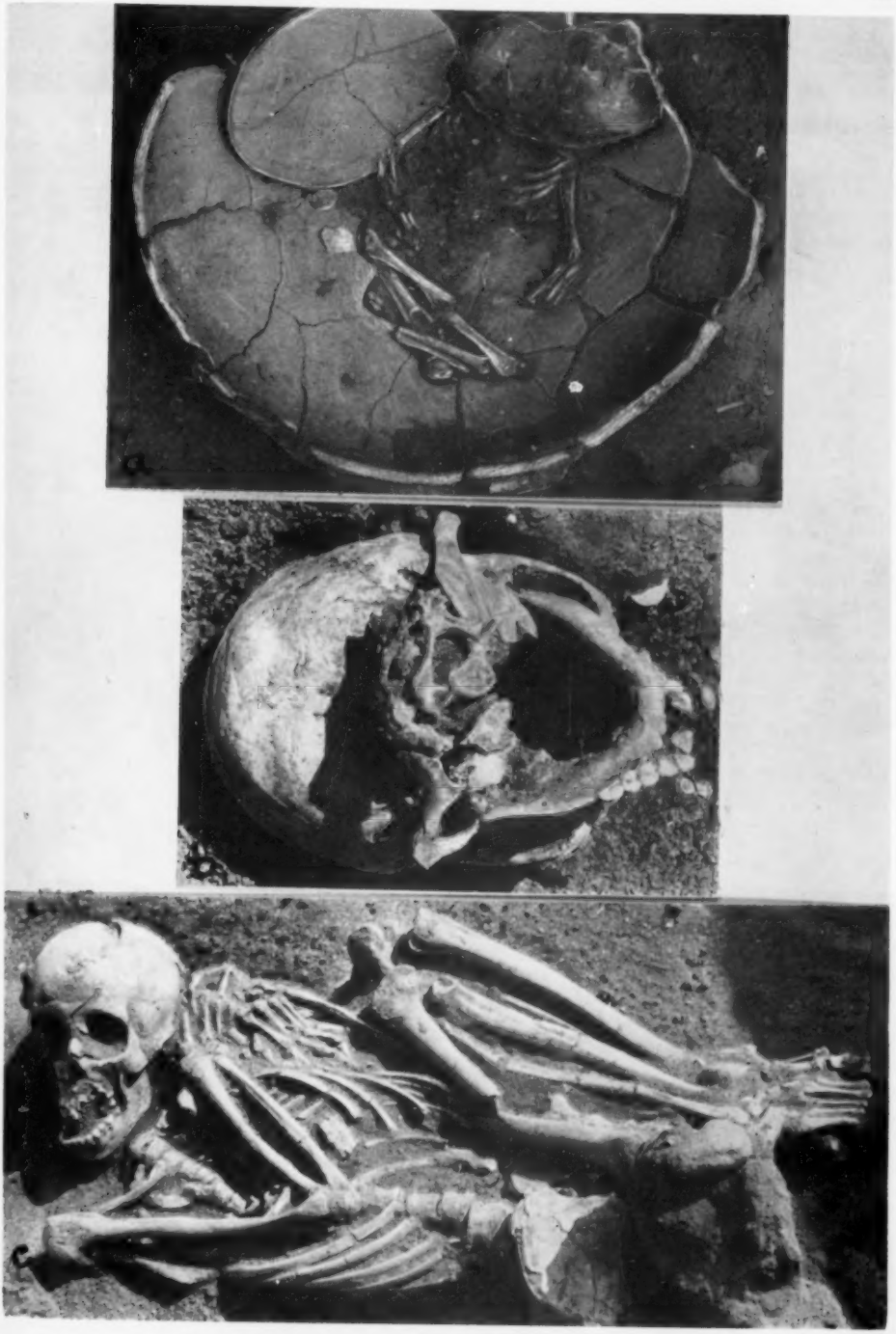


FIG. 63. *a*. Infant buried in an urn; *b*, *c*, body with a broken neck.

plete bodies from which teeth had been extracted and placed in a shell either on or beside the body. One individual was flexed on his side with a single tooth a few inches in front of his face.

Another type of ritual mutilation was observed on bodies completely articulated except for an arm which had been cut off at the shoulder. In one case, the cut-off arm had been placed directly over the other, making it appear that both were attached to the same shoulder. We also observed a body where both forearms and hands were in contact but one humerus was missing. This bone had been cut down to form a spatula, found in another part of the grave. Presumably this spatula had been used ceremonially during the burial rites, for it had absorbed a considerable amount of grease and was better preserved than adjacent bones. The removal of arms or legs recalls Mochica pottery from Peru, where vessels were manufactured in the form of limbs, and other body mutilations were shown. At one time, it was thought that these vessels represented punishment. It is currently believed that they may record acts of sacrifice.

In a few instances, individuals at Venado Beach had been buried in ordinary fashion, flexed or extended. Then a second body was hacked in several pieces and thrown into the grave. These butchered burials were extremely difficult to record, owing to the fortuitous intermingling of bones.

Finally, we found several cases where finger joints were chopped off. Usually the bones were placed in a shell, either on top of or beside the body. In three cases, each observed by a different excavator, the finger bones were found inside a skull. In one burial the body had been cut in several pieces and thrown in the grave haphazardly. The finger and hand bones had been separated, however, and scattered around the skull and around a disarticulated jaw with one ramus cut off. Small water-worn pebbles also were scattered around bones in similar fashion. Finger amputation, as is well known, has a world-wide distribution and is of considerable antiquity.

Harte's excavations, to which we have referred, yielded two other types of mutilation we did not see. One was a grave of unusual shape containing pottery but no bones except part of a pelvis. Another was an individual whose legs had been severed, one at the knee and one four inches below it. He apparently had survived this operation long enough for the bones to become pitted with infection. This unfortunate man must have been of enormous height as he measured 6 feet, 1½ inches after the amputation. He probably was a captive slave from the Escoria region, where the bearded men stood much taller than the Spaniards.

The abnormal burials at Venado Beach which we have described comprise about a fifth of the total (Table 8). About a third of all burials were flexed symmetrically on their backs with the hands under the chin, knees over the body and arms, and the feet crossed. They were so tightly compressed that they must have been roped or sewn in sacks (Fig. 64, *c*). Bodies flexed on their sides had not been compressed (Fig. 64, *b*). Only an eighth of the bodies lay in an extended position, usually on their backs (Fig. 61).

TABLE 8. ANALYSIS OF VENADO BEACH BURIALS

| Types | Harte | Lothrop | Totals | % |
|---|---|---|---|---|
| Flexed | *80* | *103* | *183* | 50 |
| on back | 52 | 69 | 121 | 33 |
| on chest | 6 | 2 | 8 | 3 |
| on right side | 10 | 16 | 26 | 7 |
| on left side | 12 | 16 | 28 | 7 |
| Extended | *41* | *10* | *51* | 14 |
| on back | 32 | 10 | 42 | 11 |
| on chest | 6 | — | 6 | 1.9 |
| on right side | 2 | — | 2 | 0.6 |
| on left side | 1 | — | 1 | 0.2 |
| Sitting burials | 3 | — | 3 | 0.8 |
| Infant burials | — | *13* | *13* | 4.5 |
| Bundle burials | *4* | *20* | *24* | 6.5 |
| Urn burials | *30* | *32* | *62* | 17 |
| Mouth open — alive? | — | 8 | 8 | 2.0 |
| Mutilations | *20* | *51* | *71* | 19 |
| head only | 3 | 2 | 5 | 1.4 |
| no head | 1 | 7 | 8 | 2.2 |
| jaw missing | 2 | 3 | 5 | 1.4 |
| arm missing | 1 | 5 | 6 | 1.9 |
| foot missing | 1 | 5 | 6 | 1.9 |
| other bones missing | 4 | 9 | 13 | 3.5 |
| teeth extracted | 1 | 9 | 10 | 2.7 |
| finger cut off | 1 | 2 | 3 | 0.8 |
| fingers in skull | 1 (+2) | — | 3 | 0.8 |
| butchered | 1 | 3 | 4 | 1.0 |
| decapitated | 2 | 1 | 3 | 0.8 |
| back broken | — | 3 | 3 | 0.8 |
| neck broken | — | 3 | 3 | 0.8 |
| Totals | *167* | *202* | *369* | |

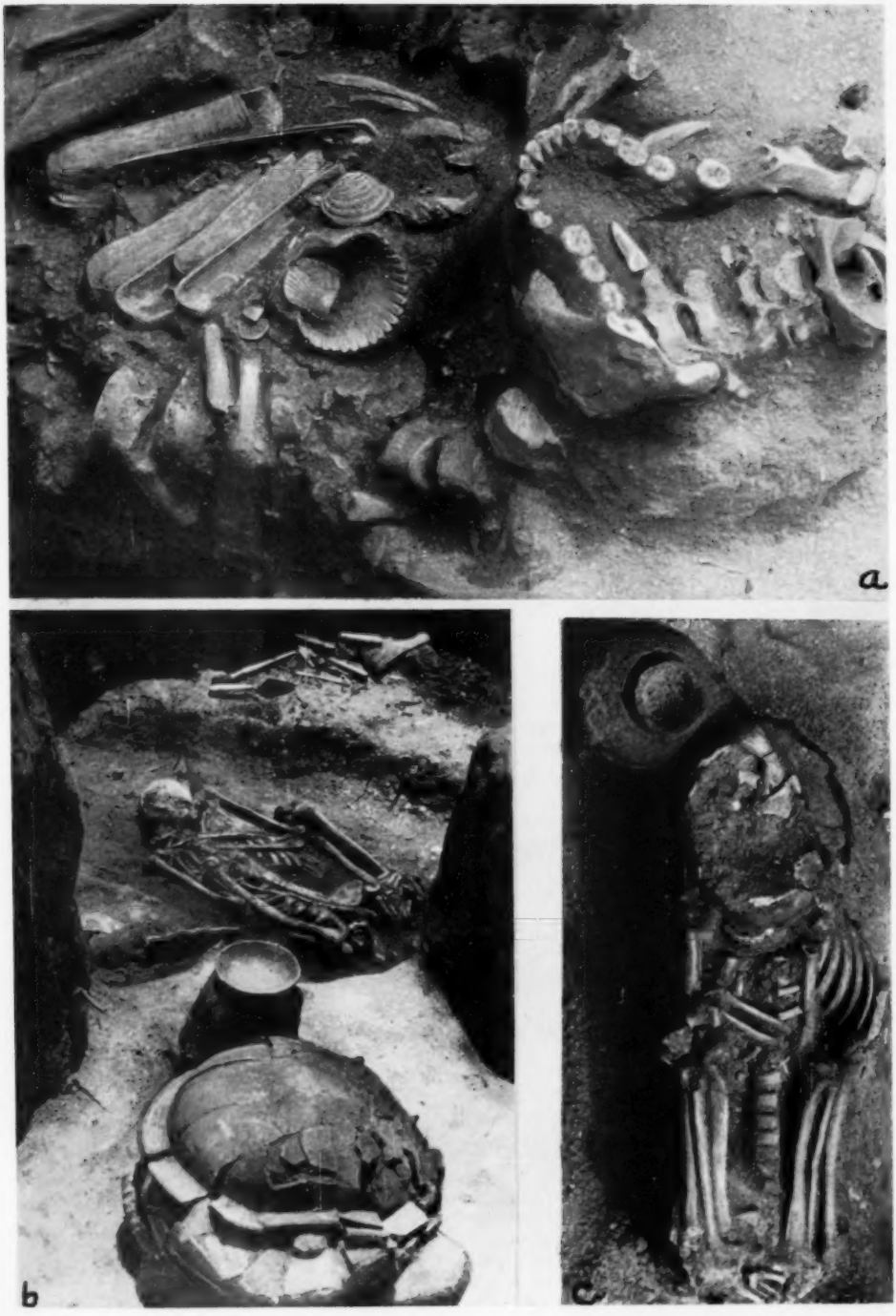



FIG. 64. *a*, Body with the cranium removed but neck and jaw intact; *b*, flexed body accompanying a rich child buried in an urn; *c*, compressed flexed body.

In conclusion, the archaeological evidence from Venado Beach in Panama indicates that many individuals either were killed or buried alive. Historical sources reveal that this was done in order that their souls might accompany their masters to serve in another world. History, however, is silent in regard to the mutilations, which occurred with such frequency that we can group them by types. We do not know their purpose. Tooth extraction and finger cutting might have been practised on the living, but other mutilations either were performed on the dead or must have caused immediate death.

Panama is not a large area and the native political units in it were small. Nevertheless, social levels were complex and comparatively rigid. The rulers are pictured primarily as warriors, and there is no historical evidence of a theocratic class. Yet it is clear that a minority dominated their inferiors in life and successfully imposed a belief that their rituals alone could open the doors to any future existence after death.

Peabody Museum
Harvard University
Cambridge, Mass.
March, 1953

# THE POINTED MOUNTAIN SITE NEAR FORT LIARD, NORTHWEST TERRITORIES, CANADA *

RICHARD S. MACNEISH

DURING THE last four years (1949-1952) the National Museum of Canada has carried out archaeological investigations in the Northwest Territories of Canada,** the primary purpose of which was to determine what kind of archaeological materials and what sort of prehistoric artifact complexes existed in that area. Since part of this region, mainly the Mackenzie River drainage, was supposedly free of ice early in the recession of the last glaciation, it has often been suggested as a possible migration route through which early groups of Indians could have passed when moving from Asia to the more southerly regions of the New World, and, therefore, it was hoped that archaeological information resulting from research in that district might throw some light on these early migrants.

The first three seasons of research (1949-1951) in northwest Canada were mainly devoted to archaeological reconnaissance (MacNeish, 1951; 1953). The scarcity of cleared land and of adequate transportation seriously hampered the survey. However, in spite of these unfavorable circumstances, artifacts were found in seventy-one different localities. The sites were scattered over a vast territory and yielded a wide range of tools. Many of the occupation sites cover but a small area, where only a few tools or chips were to be found. With the exception of a few very late ones along the Mackenzie River, most of the sites are situated on modern or extinct beaches around lakes. These lake beaches, either around the small lakes in the river valley or around the larger lakes such as Great Slave and Great Bear, are usually partially, if not totally, covered by moss, humus, or rotted vegetation, so it was necessary to sink numerous test pits at many places in order to obtain archaeological specimens.

At the end of the 1951 fieldwork, an analysis of the artifacts was undertaken. This study tentatively defined artifact complexes in the Northwest Territories but more important, it redefined the problems of the region.

This paper is concerned with one set of these problems. The assemblage from N.W.T.42, the Pointed Mountain site, included lamellar flakes, burins, snub-nosed end-scrapers, and a series of large bifaces, which in a general way were similar to artifacts found over a very wide area, extending from Kluane Lake in the Yukon (Johnson, 1946, pp. 183-87), the Campus site at Fairbanks, Alaska (Rainey, 1939), and Cape

* Published with the permission of the Chief Curator, National Museum of Canada.

** I should like to express my gratitude to Douglas Leechman, Senior Archaeologist at the National Museum of Canada, and F. J. Alcock, Chief Curator of the National Museum of Canada, who helped plan the program of research, and later made it possible to carry out this project. Also, I should like to thank the people of the Northwest Territories for their unfailing hospitality, advice, and co-operation during the survey.

Denbigh at Norton Sound in Alaska (Giddings, 1951), Cape Dorset sites (Harp, 1951), as well as two sites in outer Mongolia (Maringer, 1950; Nelson, 1926, pp. 146-51), and Siberia (Okladnikov, 1950). The Pointed Mountain industry and its relationships posed a whole series of problems concerned with prehistoric cultural relationships between northeastern Asia and northern North America. Did artifact complexes in North America show sufficient resemblances to others in northeast Asia to indicate a genetic relationship? If so, when were such relationships established and what kind of relationships were they? Again, if some cultures in North America did show numerous similarities to cultures in Asia, there still remained the question of how that American culture was related to both earlier and later cultural manifestations in its own local area.

Because of the apparent widespread relationships of the Pointed Mountain site and its relative richness, it was decided to excavate it during the early part of the summer of 1952. It was hoped the site might yield enough specimens to make it possible to describe a definite culture, which would be valuable in making comparisons with other assemblages and indicating genetic relationships. It was not expected that these investigations would solve completely the problems referred to above, but it was hoped that excavation would produce evidence relevant to their solution.

Pointed Mountain (N.W.T.42) is situated about 20 miles north of Fort Liard in the southwest corner of the Northwest Territories of Canada (Fig. 65). It is along the western side of a small valley 2½ miles north of Fishermans Lake at the foot of the eastern slope of Pointed Mountain. The site and Fishermans Lake are between two low spurs from the main chain of the Rocky Mountains that point toward the southeast. The prehistoric occupation is on top and along the upper side of a lateral moraine against the eastern slope of Pointed Mountain. The eastern edge of this moraine has been considerably eroded but aerial photographs show that it once paralleled the edge of the mountain fairly closely. The top of the moraine is more or less level and its western and northern portions gradually merge into the edge of the mountains. The eastern and southern flanks of the moraine are steep and slope down to a swampy lake-filled valley. This flat swampy valley extends from just north of the site to Fishermans Lake.

Pointed Mountain itself is composed of carboniferous sandstone, shale, and chert, and was uplifted at the end of the Mesozoic (C. O. Hage, 1935). The moraine on which the site lies was formed during the Pleistocene by ice of the Keewatin mass that pushed into this area from the east (E. D. Kindle, personal communication). E. D. Kindle, from a study of aerial photographs and a geological knowledge of this general region, believes that the valley in front of the site that extends to Fishermans Lake was once a lake (probably at the time the site was occupied) and has become filled by silt from the two small streams that enter the north end of Fishermans Lake.

Excavation at Pointed Mountain revealed a complex stratigraphy, particularly in regard to soils[1] (Fig. 65). The whole site is covered by a 4 to 8 inch layer of moss, rotted vegetation, and dark humus. This contains no cultural remains. Underneath the humus, along the upper two-thirds of the eastern and southern slopes of the moraine and in various spots on top of the bank (always above reddish-brown clayish loam $B^2$ horizon), was a pale-brown loam which is called the $A^2$ horizon. The $A^2$ horizon had its maximum thickness (of about one foot) halfway down the bank and thinned out both above and below this point. This stratum is without artifacts or chips. Below the $A^2$ horizon, along the slopes and below the spots of pale-brown loam on top of the moraine, was a reddish-brown clayish loam stratum from 2 to 6 inches in thickness called the $B^2$ horizon. In many places on top of the moraine where the $A^2$ horizon was absent, the reddish-brown loam was directly underneath the humus but in a few places it was totally missing. With the exception of the spots where horizon $B^2$ was totally absent, artifacts came from this layer only and the one cultural feature found, a hearth, occurred in this stratum along the side of the bank. Underneath horizon $B^2$ was a pale-brown clayish loam, the $B^3$ horizon, which, in a few patches on top of the bank (where $B^2$ was absent), directly underlay the humus. Only in these areas did it contain chips or

[1] I should like to thank A. Leahey, Principal Pedologist of the Department of Agriculture of Canada, for his identification of the soils, and analysis and interpretation of the soil profile.

artifacts. In a few spots on top of the moraine it was totally absent.

The maximum thickness of the $B^2$ horizon is about 16 inches; this was at a point about halfway down the bank. This lower pale-brown stratum gradually blended into a pale-brown sandy loam which contained gravel in increasing amounts as one dug deeper, called the D horizon. In a few places on top of the bank, the D horizon underlay the humus and only at these spots did chips or tools occur in it or on it. Before leaving the study of stratigraphy at the site, it is necessary to speak of a layer that is not directly connected with the human occupation. This soil is a grayish-black clayey loam which underlay the humus and, at least in part, overlay zone D along the lower third of the bank. At the foot of the bank it was the only soil under the humus. This is called the X horizon and seems to be mainly waterlaid. No artifacts were associated with it.

Reports by, and discussions with, E. D. Kindle of the Geological Survey of Canada and A. Leahey of the Department of Agriculture of Canada, have made it possible to reconstruct the history of the Pointed Mountain site and estimate the time at which it was occupied. The moraine on which the site lies and the D horizon of soil were formed during the Pleistocene. On the basis of discussions of the soil samples with Leahey, it would appear that following the Pleistocene, when the permafrost had left the surface of the ground and before the forest invaded the area, loess was blown on to the moraine. It was during the latter half of the loess blowing period that the site was occupied by man. The area at this time was probably either covered by grass or tundra, or was bare ground, and the climate was dry. Evidently at this time a lake occupied the valley in front of the site and extended a third of the way up the bank on which the site is located since only the waterlaid X horizon is found in that area and it is not under deposits of loess. Next the forest invaded the area, changing the loess into a mature Grey Wooded soil by leaching. Also, the lake in front of the habitation area filled. Finally, either the site was eroded, removing sections of the Grey Wooded soil profile or some of the soils flowed down the hill by solifluxion. Then humus was laid down over the site and the valley in front of it. At present, four kinds of evidence can be brought to bear to determine the age of the site. One is based upon the rate of deposition of the valley fill in front of the site, that is, how long did the two small streams take to fill 2½ miles of the lake that existed when the Pointed Mountain site was inhabited. E. D. Kindle estimates that it might have taken between 5,000 to 10,000 years. The second line of evidence comes from a study of the soil formation in which the artifacts were found. Leahey has indicated that the artifacts were laid down during loess accumulation before the encroachment of the forest. Raup (1947, p. 95), in studying the botany of the southwestern Mackenzie, concludes that the forest invaded the mountain areas during the early part of the altithermal period "about 7,000 years ago," so the artifacts would have been deposited before that date, say about 8,000 years ago. A third form of evidence is the age of the deposition of loess deposits in Peace River town region (an area with a Grey Wooded soil profile), a semi-wooded flat plain that extends northwest to Fort Liard. Loess deposits in this region are thought to have been formed during the postglacial optimum or altithermal. Elsewhere the altithermal has been dated as from 4,000 to 7,000 years ago. If the Pointed Mountain artifacts in the upper third of the loess deposits were deposited at the same time as the loess at Peace River, a date of 5,000 years ago would not be unreasonable. In the Yukon around Kluane Lake, Johnson and Raup have studied soil profiles and have attempted temporal correlations, and I have been able to consult them regarding these problems. Though this region is in the St. Elias Mountains and not next to the Rockies, and the type of soil profile is different from that found at Pointed Mountain, the sequence of climatic changes and soils seems to be similar to those at Fort Liard. The earliest soil strata in that area, as at Pointed Mountain, are of glacial origin. In the Yukon these were covered by a windblown loess that matured to form the Kluane silts. This occurred, according to Raup, during the postglacial optimum, 5,000 to 7,000 years ago, when the area was mainly tundra or grassland. This period of deposition seems analogous to the period of loess deposition at Pointed Mountain and is also the period of the use of microliths and polyhedral cores by the local populations.

The period of deposition of Kluane silts is followed by a period of stability in which a humus was formed and then a cold period with erosion, perhaps comparable to the period of

dissection of the silts at Pointed Mountain. Next came a period of volcanic activity with deposition of more silts, and the invasion of the forest in the Yukon, which may be analogous to the deposition of the humus, invasion of the forests, and the beginning of a new soil profile at Pointed Mountain. If this analogy between Pointed Mountain and Kluane Lake is correct, then human habitation probably occurred at the former between 5,000 to 7,000 years ago.

The four lines of inferential evidence all point to a dating of 5,000 to 8,000 years ago for the occupation of the Pointed Mountain site. Fortunately carbon was found here in a fireplace associated with the artifacts, so eventually a fifth kind of evidence for dating, of a more conclusive nature, may be used.

## The Site

The site, when first discovered in July, 1950, was covered with dense underbrush and tall pines except for a narrow path across its western extremity. On this narrow path on top of the moraine one of my workers, Charlie Cholo, found an obsidian lamellar flake. During the next four hours three of us stripped off sod and dug with trowels in the underlying reddish soil on either side of the path in an area of about 50 square feet. Circumstances made it necessary to leave the site after that brief testing.*

In June, 1952, I returned to Fishermans Lake and for the first five days, besides preparing for extensive excavation, I endeavored to train workmen by digging a late site on the shores of that lake. When I felt that the workmen were sufficiently skilled, we moved to the Pointed Mountain site and cleared an area 100 by 20 feet along the eastern portion of the site. Then a 15 by 90 foot north-south trench composed of 5-foot squares was staked out in the cleared region just north of the greatest artifact concentration found in 1950. Excavation was done with trowels; great care was used in recording provenience of specimens.

In my study of the artifacts, after they had been washed, catalogued, and numbered, a comparison of tools from various areas at the site was undertaken. That is, artifacts from the $B^2$ horizon on top of the moraine were compared with those from the $B^2$ horizon from the sides of the slopes, with those from the $B^3$ horizon on top of the bank where the $B^2$ horizon was absent and with those from the D horizon where the $B^2$ and $B^3$ horizons were absent. This set of comparisons revealed that there were no significant differences between artifacts in the different areas of the site and that in all probability all artifacts were from a single occupation by a single people. Furthermore, it indicated that the $B^2$ horizon, which contained most of the tools and chips, was the zone of occupation and that those objects found in the $B^3$ and D horizons (where the $B^2$ horizon was absent) probably represent artifacts that originally were in the $B^2$ horizon but which had been re-deposited in the lower strata when the reddish-brown clayish loam had been eroded or solifluxed away.

The next step in the analysis of the artifacts was to divide them into 6 categories based upon their supposed uses as indicated by ethnographic information or comparative archaeological data. These classes are: Polyhedral cores, lamellar flake insert blades, engraving tools, projectile points, scrapers, and bifaced blades.

The artifacts of each of these classes, either individually or as groups of artifacts with similar forms and features, were then compared with artifacts or photographs or drawings of artifacts from various sites in the surrounding area and from sites in Asia and North America that displayed artifactual similarity. Upon the basis of their significance in time and space, the Pointed Mountain artifacts were divided into a series of artifact types. In a few cases, for convenience in description, I divided the artifact types into subtypes.

After the types had been determined, an attempt was made to compare the entire complex of types from the Pointed Mountain site with the complete complex of types from other sites in order to ascertain possible prehistoric relationships. At present there are not many similar trait complexes in the north.

* I am particularly indebted to Johnny Klondike who first guided me to this archaeological zone, who later assisted in the excavations, and who was a most hospitable host for my workers and me while at Fishermans Lake. I should also like to thank Charlie Cholo and Walter McPherson who were my companions during the survey, as well as St. Pierre Moyah, Alexis Behile, Francis Adah, Phillip Adah, and Francis Moyah, who helped dig the site. I am extremely grateful to Constable Harry H. Aime of the R.C.M.P. and his wife, Mrs. V. J. Aime, John Cunningham of the Hudson's Bay Company, J. Griffin, "Buster" Brown of Socony Vacuum Company, and Willy McLeod, all residents of Fort Liard in 1952, who made my work both pleasant and profitable.

Before artifacts themselves are discussed, it seems pertinent to comment on the materials used as demonstrated by both the chips and the artifacts, as I believe some valuable inferences may be made from studying them. Table 9 illustrates the materials at the site. From the large quantity of chips and nodules, I believe it is safe to conclude that the Pointed Mountain site was a workshop as well as a habitation area. The kinds of materials shown on Table 9, lines 1 to 6, are all local chert to be found at Pointed Mountain. Lines 7 to 11 seem to be chert from pebbles that also are of local origin. However, obsidian is not found in this region; the nearest source now known is in Dezade ash area in the Yukon, near Whitehorse. Furthermore, there are more complete artifacts of obsidian than chips and there are no nodules. Thus I believe the Pointed Mountain people certainly had contact into the Yukon and that they either received (by trade?) whole tools from the Yukon or they may have brought

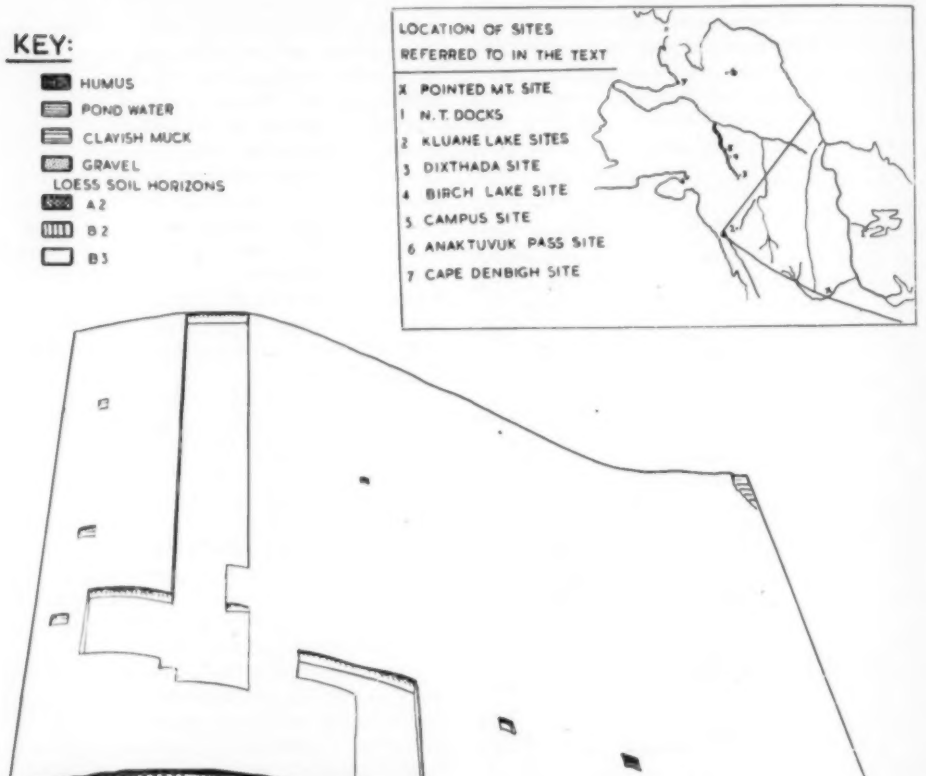



FIG. 65. Isometric drawing of Pointed Mountain site looking northward with insert map of northwest America and pertinent sites.

whole tools with them when they moved in from the Yukon.

Most of the artifacts (about 66%) are lamellar flakes that have been struck from previously prepared polyhedral cores. In spite of the large numbers of lamellar flakes, the polyhedral cores from which they were struck are few (8) in number; cores are of two types.

*Crude sub-conoidal polyhedral cores* (See Fig. 67, 3). These 4 cores are "nondescript spalls perhaps already provided with a thick flat edge serviceable as a striking platform without much additional preparation" (Nelson, 1937, p. 271). They are roughly sub-conoidal in shape with lateral fluting on one side and the striking platform seems to be an accidental break rather than intentional. They show considerable range in size, the biggest being 42 mm. in diameter and 35 mm. long, while the smallest is roughly 30 mm. in diameter and 10 mm. long. Such crude cores have a wide temporal and spatial range throughout the world.

*Tongue-shaped polyhedral cores* (See Fig. 67, 4). These 4 cores are roughly rectangular or tongue-shaped in outline; in cross-section they are more or less oblong. One of the shorter oblong ends served as a striking platform; the opposite end is narrow and often shows retouching along one or both of its surfaces, giving it a rough cutting edge. Two of the broader sides are usually flat while the narrower polyhedral sides have fluting on one or both of them. The largest is 39 mm. long, 22 mm. wide, and 7 mm. thick, and the smallest is 13 mm. long, 11 mm. wide, and 6 mm. thick.

Maringer (1950, p. 173) has previously pointed out that this specific type is not known west or south of Mongolia but occurs in inner and outer Mongolia, Manchuria, Sinkiang Province of China, the Lake Baikal region of Siberia, Kamchatka, and at the Campus site (Nelson, 1937). To these I might add a site on the Chuckchi Peninsula (Krader, 1952, p. 261), the Birch Lake site of Central Alaska (Skarland and Giddings, 1948, Fig. A, *i*), and the Pointed Mountain site of the Northwest Territories of Canada. In northeast Asia this type appears in Mesolithic and early Neolithic, 4,000 to 8,000 years ago (Maringer, 1950, p. 175).

The 251 lamellar flakes or bladelets were struck off from previously prepared cores of the types described above. They all have a roughly prismatic form and range from some only 10 mm. long, 2 mm. wide, and ½ mm. thick to others 57 mm. long, 20 mm. wide, and 8 mm. thick. Inferences concerning the use or uses of such bladelets are based upon finds in the Mesolithic of Northern Europe and in Siberia. In the above areas some lamellar flakes have been found inserted in slots in bone or wood and they are thought of by Clark (1938) as knives, daggers (p. 151 Fig. 55, 6), barbs for leister prongs (p. 115), barbs of projectile points (arrows or spears) (p. 116, Fig. 41, *21-5*), and the points of projectile points (p. 201, Fig. 69, *27*).

On the basis of the different forms of, and techniques used upon, lamellar flakes, in conjunction with comparative data, it has been possible to divide them into a series of types.

For convenience in description I have divided the (218) unretouched lamellar flake type into three groups. The first group I called *Unretouched Rectangular Lamellar Flakes* (Fig. 66, 3, 4). This group is represented by 102 specimens. Cruder ones (48) have flat concave or convex ventral surfaces; the dorsal surfaces bear one or, rarely, two longitudinal ridges that are usually not straight and only roughly parallel to the edges of the flake. They are rectangular in outline. They range in size from 8 mm. long, 3 mm. wide, and 1½ mm. thick

TABLE 9

| Kinds of Materials | Cores | Flakes | Polyhedral Cores | Flakes Lamellar | Tools Engraving | Points Projectile | Scrapers | Tools Biface | Totals |
|---|---|---|---|---|---|---|---|---|---|
| 1. Bluish-black chert | 25 | 549 | 6 | 104 | 16 | 2 | 48 | 2 | 752 |
| 2. Bluish-gray chert | 4 | 204 | — | 28 | — | — | 3 | 2 | 241 |
| 3. Dark gray chert | 1 | 71 | 1 | 20 | 3 | 2 | 31 | 1 | 129 |
| 4. Mottled dark blue light blue chert | 8 | 617 | — | 31 | — | 2 | 20 | 6 | 684 |
| 5. Mottled blue-gray chert | 5 | 329 | 1 | 16 | 1 | — | 11 | 7 | 370 |
| 6. Mottled dark blue brown chert | 19 | 388 | — | 19 | 1 | 4 | 10 | 11 | 452 |
| 7. Mottled pink-gray chert | 3 | 47 | — | — | — | — | — | — | 50 |
| 8. Mottled brown chert | 1 | 27 | — | — | 3 | — | — | — | 31 |
| 9. Brown chert | — | 12 | — | 1 | — | — | 6 | 1 | 20 |
| 10. Milky gray chert | 1 | 9 | — | 1 | 3 | — | — | 2 | 16 |
| 11. Speckled reddish chert | 1 | 7 | — | 7 | — | — | — | — | 15 |
| 12. Obsidian | — | 3 | — | 24 | — | — | — | — | 27 |
| Totals | 67 | 2263 | 8 | 251 | 27 | 10 | 129 | 32 | 2787 |

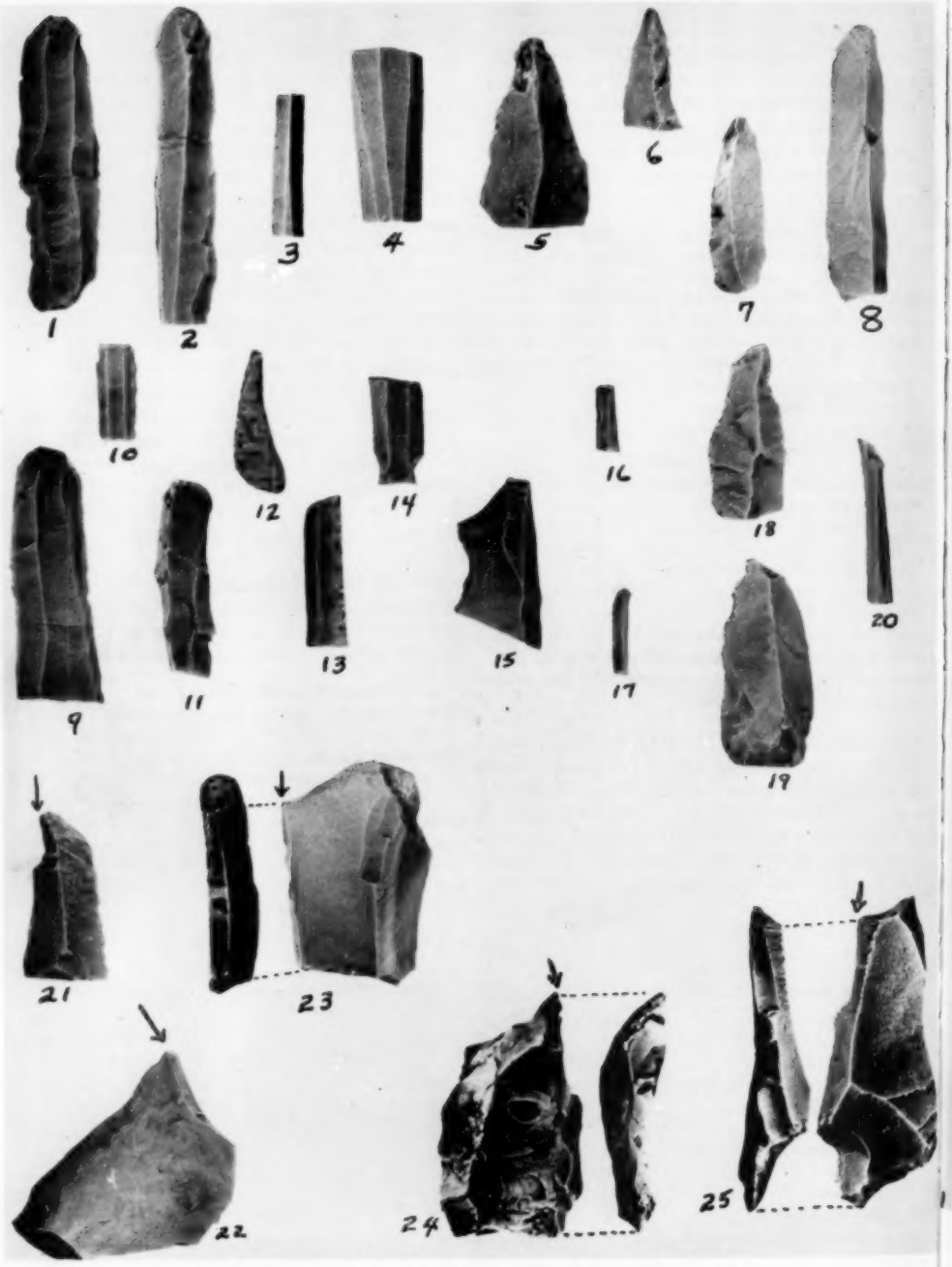



FIG. 66. Lamellar tools and burins from Pointed Mountain. Full size.

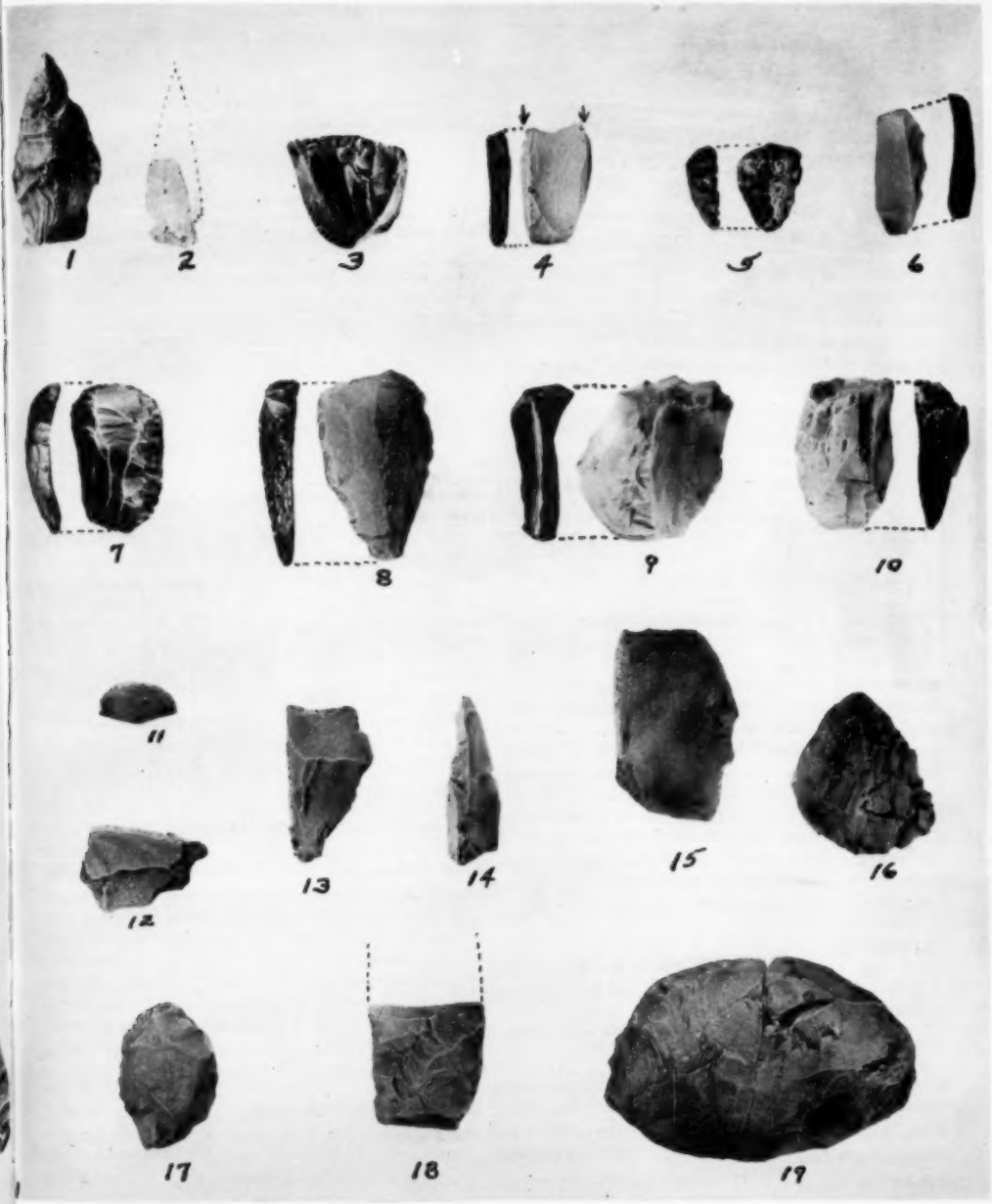



Fig. 67. Heavy stone tools from Pointed Mountain. One-half natural size.

to 39 mm. long, 21 mm. wide, and 6 mm. thick. They were evidently struck from poorly prepared cores of poor quality chert with irregular fluting. The better made rectangular lamellar flakes (54), with straight dorsal ridge or ridges are made from dark chert or obsidian. These materials have good conchoidal fractures, thus allowing the manufacture of more regular fluted cores. More than half the specimens are quite small, the smallest being 7 mm. long, 2.5 mm. wide, and ½ mm. thick, while the largest is 44 mm. long, 12 mm. wide, and 3 mm. thick.

When I first separated this group of tools, I thought of them as broken lamellar flakes with rounded ends from which the bulb of percussion had been broken off accidentally, or as merely waste flakes. However, some of their edges show definite wear. Moreover, only bladelets such as these, when inserted into slots, one next to the other, would give a continuous cutting edge for a knife or a dagger. Of course, they could be used as barbs in projectile points (spears), leister prongs, or as the teeth of saws like other micro-blades. Therefore, I have concluded that though many of this variety were parts of tools, some had perhaps been purposely broken to give them a rectangular outline.

The second group includes (89) *Unretouched Rounded Lamellar Flakes* (Fig. 66, *1, 2*). These microliths usually have a flat to very slightly concave ventral surface with a bulb of percussion near the rounded end. The dorsal surfaces of 59 of them have one or two straight ridges parallel to the edges while 30 have one or, rarely, two irregular ridges. One end of the dorsal surface (just opposite the bulb of percussion) is usually rounded. The rounding and the fine flaking at the rounded end are due to the preparation of the core near the striking platform before the bladelet was removed. The opposite end is usually broken off sharply at right angles to the long axis of the flake. In three cases was I able to fit together the squared ends of rounded flakes to form long oval flakes, and therefore suspect that flakes were purposely broken to form a squared end. Small nicks due to wear or use are to be found along one edge of many specimens of this kind of flake. Specimens vary from 10 mm. long, 2 mm. wide, ½ mm. thick, to 57 mm. long, 20 mm. wide, and 5 mm. thick.

Three specimens were found at a depth of 8 inches in the reddish silt in the $B^2$ horizon. They lay in a line, with a half-inch space between them, with their rounded ends all pointing in one direction. From the position of these specimens and from information from the Mesolithic of Northern Europe (Clark, 1936, p. 116, Figs. *21-25*), and the early Neolithic of Lake Baikal (Okladnikov, 1950, Pl. *31*), I believe the round-ended lamellar flakes were insert barbs for arrows, spears, saws, or leister prongs.

The final group, called *Unretouched Pointed-end Lamellar Flakes* (Fig. 66, *5, 6*), is represented by 27 specimens. The ventral surfaces are usually concave though 8 are convex and 3 are flat. Dorsal surfaces usually bear a single medial ridge. Bulbs of percussion are usually not apparent (17) but a few (7) have bulbs near their points and 3 have them at their wide bases. In general outline they are isosceles triangles. Most of those with concave ventral surfaces are probably struck from more or less conical or sub-conoidal polyhedral cores while the others may be from any sort of irregular polyhedral core. There is no great range of size; lengths range from 38 to 11 mm., widths from 18 to 5 mm., and thickness from 5 to 1 mm.

At first glance these seem to be accidentally-formed triangular flakes of flint, but most of them are thinned near the base so they are not broken from larger flakes. Furthermore, some of them have use scars along the edges, and one looks like it has a retouched pointed end. Therefore it is probable that at least some of them served as tools. They may have been used as insert points for projectiles or as barbs.

Unretouched lamellar flakes of all three forms seem to have a world-wide distribution and to occur in many periods after the Paleolithic so they have no great significance in either time or space. In the Northwest Territories they occur in two other complexes beside that of Pointed Mountain, namely, the nearby Fishermans Lake complex and the Franklin Docks complex on Great Bear Lake.

*Lamellar Flakes with Retouched Rounded Ends* (Fig. 66, *8*). These 6 flakes have flat ventral surfaces with a poorly-defined bulb of percussion near their rounded end. The dorsal surfaces have either a single longitudinal ridge or a flat central longitudinal plateau with sides sloping off sharply from the two ridges bordering the central plateau. One end is usually broken off sharply at right angles to the main axis of the blade while the other end is convex in outline. Dorsal surfaces at the rounded end usually slope up from the ventral surface at about a 45° angle and blend into the raised portion of the dorsal surface. This slanting dorsal end bears a series of minute pressure flake scars (between .25 and 1 mm. in width). The ventral surface of one specimen shows delicate retouching. This type ranges in size from .7 mm. thick, 3 mm. wide, and 11 mm. long, to 3 mm. wide, 9 mm. wide, and 44 mm. long.

At no site have similar types been found in a context that would indicate their use but one might surmise that, like other lamellar flakes,

they were used as insert blades for projectile points. In North America similar types are found at both Cape Denbigh (Giddings, 1951, p. 149, Fig. 61, *b*, 5) and at the Campus site (Rainey, 1939, Fig. 8, 3). In collections of the Mesolithic and Neolithic and outer Mongolia at the American Museum of Natural History I have seen similar types; Okladnikov illustrates similar ones from the Lake Baikal Neolithic and Mesolithic (Okladnikov, 1950, Pl. 17, lower left-hand corner).

*Pointed Lamellar Flakes with Retouched Rounded Ends* (Fig. 66, *7*). These 3 artifacts have slightly concave ventral sides with ill-defined bulbs of percussion near the pointed ends and appear to have been struck from a sub-conoidal type of polyhedral core. Dorsal surfaces have a single longitudinal ridge. One end of the bladelets is pointed without retouching while the opposite end is rounded and retouched on its dorsal edge. Wear flakes appear along the edges. All of them are about 25 mm. long, 8 mm. wide, and 2 mm. thick.

Similar types are rare though some were found at Cape Denbigh (Giddings, 1951, Fig. 61, *b*, 3-4), and, from illustrations of the Campus site materials, at least one seems to have been found there (Rainey, 1939, Fig. 8, *10*). Such a type might have been inserted into the end of a shaft as a projectile point but proof that they were so used is lacking.

*Lamellar Flakes with Two Edges Retouched* (Fig. 66, 9). Ventral surfaces on all these are slightly concave while 3, rectangular in outline, have a single dorsal ridge and 2 with rounded ends have two dorsal ridges. Dorsal edges on the two longer sides bear fine retouching. They range between 24 to 41 mm. long, 10 to 16 mm. wide, and 1 to 2 mm. thick.

Like many other lamellar flakes, these probably served as inserted blades for daggers or knives, or as barbs of harpoons. They are present at Cape Denbigh (Giddings, 1951, Fig. 61, *b*, 7) and the Campus site (Rainey, personal communication), at N.W.T.42 and N.W.T.54 in the Northwest Territories of Canada and at Lake Baikal in Siberia (Okladnikov, 1950, Pl. 17).

*Lamellar Flakes with a Single Retouched Edge* (Fig. 66, *10*). The 7 flakes are rectangular in outline and have fine retouching along one of their dorsal edges. The ventral surfaces are usually slightly concave and dorsal surfaces have either one or two ridges on them. They range in size from 1 to 6 mm. in thickness, 6 to 17 mm. in width, and 10 to 28 mm. in length.

This type had a wide range appearing at the Fishermans Lake site near Pointed Mountain, at the Franklin Tanks site on Great Bear Lake, at Kluane Lake sites in the Yukon, at the Campus site (Rainey, 1939, Fig. 8, 4), at Cape Denbigh, Alaska (Giddings, 1951, Fig. 61, *b*, 4), sites around Lake Baikal, Siberia (Okladnikov, 1950, Pl. 17), and in sites in Mongolia (Maringer, 1950, Pl. 23, 8-9). Flakes of this variety could serve either as knife blades or as projectile point barbs.

*Notched Lamellar Flakes* (Fig. 66, *14, 15*). Seven rectangular lamellar flakes have a very small notch on their dorsal surfaces, one has a notch on its ventral surface, and one has a notch on its ventral surface just across the blade from one on its dorsal surface. The notches show 2 to 6 very fine flake scars. They range in size from one ½ mm. thick, 4 mm. wide, and 10 mm. long, to one 2 mm. thick, 13 mm. wide, and 30 mm. long.

Only from inner Mongolia have similar artifacts been reported and Maringer surmises that the notches were made so that the lamellar flake could be tied for carrying (Maringer, 1950, Fig. 38). Also Okladnikov (1950, Pl. 17), illustrates one which might be such but it is difficult to tell from the drawing.

*Retouched Semi-lunar Lamellar Flake* (Fig. 66, *12*). This flake is roughly the shape of a quarter moon. The ventral surface is slightly convex and no bulb of percussion is present. The convex dorsal edge has a steep vertical wall leading from the ventral side and is retouched along its summit. The rest of the dorsal side gradually slopes down to the concave edge. This specimen has a maximum thickness of 2 mm., is 7 mm. wide, and 23 mm. long.

This is a type restricted to the Pointed Mountain site and may have been used as a projectile point barb.

*Retouched Lamellar Flake in the Shape of a Bread Knife* (Fig. 66, *13*). This specimen has a square base 6 mm. wide, parallel edges 20 and 22 mm. long, and an oblique convex end. The ventral surface is slightly concave. The shorter side on the dorsal surface slopes up gradually to a longitudinal ridge one-third of the way across its body and the dorsal surface is flat across the rest of its width, dropping off vertically to its ventral side. The top of this vertical side is retouched as is the dorsal surface of the oblique convex end.

This specimen is unique and of unknown use.

*Lamellar Flake with a Retouched Squared End* (Fig. 66, *11*). One rectangular flake 33 mm. long, 8 mm. wide, and with a maximum thickness of 4 mm., has fine retouching along one of its shorter sides giving it a squared end.

Okladnikov illustrates artifacts of this type from the Trans-Baikal region (Okladnikov, 1950, Pl. 17), and I saw a few in the collections of the American Museum of Natural History, from outer Mongolia.

There are 27 engraving tools, possessing either sharp points or chisel-like edges which could be used for incising lines or cutting slots. Most of them are made from lamellar flakes but some of the burins are not.

*Beaked Lamellar Flakes* (Fig. 66, *18, 19*). Nine of these are struck from polyhedral cores, the other 2 are made from crude flakes. All of them have sides contracting to a point and a roughly square base. The pointed ends have a small hooked or beaked projection either to the right (5) or left (6). Six of the specimens have retouching along the edge of the same side as the beaked point, near the base. Five specimens have retouching on the convex edge of the beaked point. In size they range from 1 to 7 mm. thick, 10 to 18 mm. in maximum width, and 15 to 32 mm. long.

The ends of the points are sharp and acute, not burin shaped. Therefore, I believe the tools were used for engraving and not for cutting slots. They are restricted to the Pointed Mountain site.

*Micro-gouges* (Fig. 66, *17*). These 5 tools are made from lamellar flakes with a very definite concave ventral surface. Relatively speaking, they are thick (ranging from 2 to 4 mm.) in proportion to their width (ranging from 3 to 6 mm.). The dorsal surfaces have two longitudinal ridges outlining a central plateau. The sides, from the ventral surface to the dorsal ridges, are very steep. Both ends are more or less squared, one end being about twice the width of the other. The narrow ends slope up from the edge to dorsal plateau and are retouched.

These tools may have been used as small gouges for cutting slots and a few may be burin spalls. They are found at Pointed Mountain only.

*Micro-burins* (Fig. 66, *21*). Three rough lamellar flakes have been made into burins by striking off flakes from one corner with a blow from above the longitudinal axis. Such blows left a chisel-like edge at one end of the flake where the rectangular portions have been removed from the corner. Two of the micro-burins have slight retouching along their edges across from the notched corner. They range from 3 to 5 mm. in thickness, 14 to 15 mm. in width, and 26 to 32 mm. in length.

The only other place in the New World, at which micro-burins have been reported is Cape Denbigh, Alaska (Giddings, 1951, Fig. 61, *b*, *2*). They are, of course, diagnostic implements of the Mesolithic in the Old World and the regions nearest to the New World that they have been reported from are Lake Baikal (Okladnikov, 1950, Pl. 17), and outer Mongolia.

*Corner Burin with One Edge Trimmed* (Fig. 66, *23, 25*). There are 2 of this type with slightly different forms. One is made from a thick (6 mm.) prismatic flake with straight parallel edges, a straight end, and a steep convex end that has some retouching on it (Burkitt, 1925, pp. 68-71). This end indicates that this tool may have been also used as an end scraper. One of the edges, at its junction with the convex end, has a chisel-like cutting edge formed by four blows from above the longitudinal axis against one corner of the convex end. Thus this tool served also as a burin as well as a scraper. The tool is 34 mm. long and 22 mm. wide. The other burin is made from an irregular flake with contracting sides and pointed ends. At the point where the narrow, retouched, pointed end meets one side, a number of narrow spalls have been removed so that it has a chisel-like cutting edge. The tool could serve as a burin for cutting slots, and its retouched point could be used for engraving. It has a maximum thickness of 9 mm., a maximum width of 22 mm., and is 51 mm. long. Among the chips there are four fragments of burin spalls and some of the smaller lamellar flakes may have been spalls too.

Both these burins are typical of those "which in modified forms and sporadic distribution continued to be made throughout Mesolithic times" (Giddings, 1951, p. 194). I saw one of this type from a Mesolithic (or Neolithic) station from outer Mongolia at the American Museum of Natural History in New York, and they evidently occur in the Trans-Baikal region (Auerback and Sosnovsky, 1932, Fig. 4, *10, 13*). In the New World they were uncovered at Cape Denbigh, and in Dorset sites, though there is a general tendency for them to have finer retouching than have any of the others mentioned above (Collins, 1953). None had been previously described from the Campus site, but William Irving (personal communication) reports this type from that site.

*Angle Burin, One Edge Trimmed* (Fig. 66, *22, 24*). One burin, made from a roughly triangular flake, is 37 mm. wide, 32 mm. long, and about 12 mm. thick. One side of the apex of the burin has been struck from above, giving one side of the point a chisel edge while the other side has a few small retouchings on it.

The second burin of this type has a flat (about 10 mm. thick) rectangular body (22 mm. wide and 31 mm. long) with a triangular spur about 8 mm. long projecting from one of the shorter sides of the body. Four blows

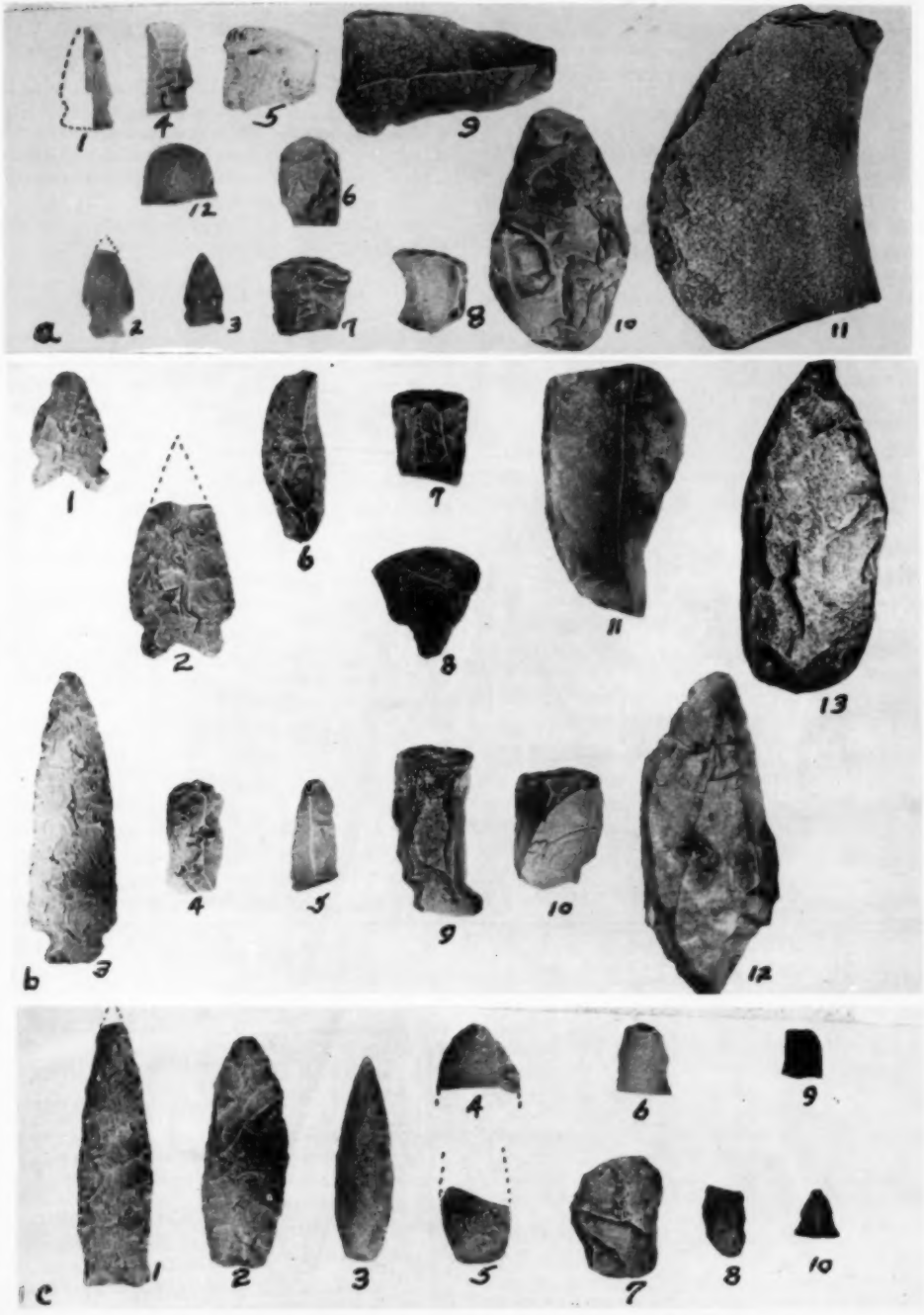



FIG. 68. Artifacts of *a*, The Spence River; *b*, The Fishermans Lake; *c*, The Sandy Lake complexes. One-half natural size.

(the longest being 15 mm.) have been struck off the spur and part of the adjacent side by blows from above the main axis of the spur and at a slight angle (30°) to the sides. These blows have left a chisel-like edge at the end of the spur on one side which allows this tool to be used as a burin for cutting slots or engraving.

Angle burins are common in Dorset sites (Collins, 1953), Cape Denbigh (Giddings, 1951), and the Anaktuvuk site (Irving, 1951, Fig. 34; Solecki, 1951, Fig. 36, *k-m*). Nelson found some in outer Mongolia, and they occur in northeast Siberia (Sosnovsky, 1933, Fig. 2, 9). This type (Irving, personal communication) also occurs at the Campus site.

*Lamellar Flake Drills* (Fig. 66, *16*). These 3 pointed prismatic flakes are between 22 and 11 mm. long, have maximum width from 3 to 8 mm., and maximum thickness from 2 to 4 mm. The top dorsal ridge of one is retouched, while all three edges of the others near the points are retouched.

This type is unique for North America but seems widespread in both Mongolia (Maringer, 1950, Pl. 29, *1-10*), and Siberia (Okladnikov, 1950, Pl. 17), and in the latter region they are considered to be perforators or drills.

*Lamellar Flake Graver* (Fig. 66, *26*). This prismatic flake of chert is 2 mm. thick, 4 mm. wide, and has one edge 26 mm., while the other is 22 mm. in length. One end is straight, the other oblique, and has fine retouching on its dorsal surface to give it a sharp point.

There were 10 projectile points. The 9 fragmentary and one whole tool of this class are narrow, bifacially chipped, with one end pointed and the other prepared for hafting. Though it perhaps is not best to draw conclusions without expert technical knowledge of archery, it would seem that these stone points are too heavy for arrowpoints and more likely were used as lance or spear points.

*Contracting Stemmed Points* (Fig. 67, *1*). The nearly whole specimen is 68 mm. long and a reconstruction reveals that it was probably about 72 mm. in length originally. The maximum thickness at the shoulders is 9 mm., while the maximum width, also at the shoulders, is 29 mm. The body of the point is roughly triangular with tapering slightly excurvate sides and a length of 45 mm. The stem is 23 mm. long and gradually tapers into a rounded base about 15 mm. wide. This point is of heavily patinated gray chert. The other fragment of this type is a basal portion of contracting stem with a rounded base.

Specimens of this type have not been recorded for the Northwest Territories or the Yukon but they do occur in Alaska. Nelson (1937) mentioned a contracting stemmed point as occurring at the Campus site, and Rainey (1939, Fig. 8, *21*) illustrated one from that site that may be of this type.

*Long Side-notched Points* (Fig. 67, *2*). These 2 basal fragments of points have slightly excurvate bases (21 mm. wide) with wide side notches just above the base. Both fragments have a maximum thickness of 6 mm. A reconstruction based on the larger fragments, made by the part of one side and the whole base as the basis for the outline of the other side, and then projecting the tapering sides to a point, suggests a point 62 mm. long.

Points similar to this type have been found at sites at Trout Lake, Bovie Lake, and at Nelson Forks near Fort Liard, as well as at the Franklin Docks site on Great Bear Lake and at sites belonging to the Lockhart River complex (MacNeish, 1951), of the Barren Lands of the Northwest Territories. They also occur in the Yukon, and in Alaska, with microliths. Nelson (1937) mentioned a side-notched point as coming from the Campus site. Thus the type seems to occur with microliths at sites in the northwest of North America as well as at sites without bladelets.

The scrapers (129 specimens) are characterized as a class by retouching along one or more edges of the dorsal surface, while the ventral surface is either flat or concave and composed of a single flake scar.

While we were digging the site, animals were hunted and their hides prepared in our camp, so it was possible to observe the skinning and scraping process of a group of Slavés as well as discuss the procedure and the possible function of some of the archaeological artifacts from Pointed Mountain. On the basis of inference from these discussions and observations from Slavé practises, the scrapers uncovered may be divided into two general classes on the basis of use: end scrapers used in fleshing and scraping the skins, and side scrapers or skinning knives used in separating the skin from the animal and in the preliminary removal of muscle tissue from the skin. Artifacts belonging to both these general classes on the basis of comparison may be divided into a series of scraper types.

*Large Plano-convex End Scrapers* (8) (Fig. 67, 9). These are made from cracked nodules of chert with more or less flat ventral surfaces and irregular convex dorsal ones. They vary in thickness from 18 to 30 mm. In outline they are roughly oblong, the smallest measuring 45 mm. by 33 mm. while the largest is 59 mm. by 56 mm. One of shorter dorsal sides show retouching. On the five larger specimens there is a series of long deep percussion flakes away from the edge on the dorsal

surfaces overlaid by scars of pressure flaking right at the edge. These larger flakes give the cutting edge a serrated appearance.

This type of scraper appears only at Pointed Mountain in the Northwest Territory though similar tools are found with microliths at Kluane Lake in the Yukon, Natalkuz Lake site in British Columbia, at the Campus site and at Dixthada site in Alaska (Rainey, 1939, Fig. 5, *14*). Similar tools occur near Lake Baikal at the Verkholenskaia Gora site (Field and Prostov, 1937, Fig. 2, *25*), as well as in central Mongolia (Maringer, 1950, Pl. 27, *26*).

Scrapers of this type were probably held in the hand for scraping and were not hafted.

*Peaked End Scrapers* (2) (Fig. 67, *10*). One whole and one broken specimen of this type were uncovered. Both are flat ventrally. Dorsally they rise to a single central peak about 15 mm. high. They are oblong in shape and bear fine retouching along their edges with slightly larger and more pronounced pressure flaking appearing along one of the shorter edges. The whole specimen is 31 mm. wide and 50 mm. long and the fragmentary one was probably the same size.

This type is also found at the Fishermans Lake site, but does not seem to appear elsewhere in the north. It probably was held in the hand for scraping.

*Large Tear-drop shaped Snub-nosed End Scraper* (See Plate II, Fig. 67, *7, 8*). There are 4 whole specimens and 6 fragments of this type. Ventral sides are always concave. Dorsal surfaces show the wide slightly convex ends to be steep with pressure flaking on them. The lateral edges are slightly excurvate, retouched, and gradually slope into more or less flat ventral surfaces. The scrapers range between 48 to 71 mm. in length, have a maximum width between 28 and 35 mm. and are between 4 to 10 mm. in thickness.

Scrapers of this type were probably hafted and used for both fleshing and scraping. The type is quite widespread on an early time level in northern North America and northeast Asia though it is lacking at Cape Denbigh.

*Small Triangular Steep-sided End Scrapers* (Fig. 67, *5*). There are 5 complete artifacts and three fragments of this type. Ventral surfaces are uniformly flat while dorsal surfaces have retouching all over them and the three sides are steep, rising to rounded keel-like prominence. In outline the scrapers are roughly triangular with slightly excurvate edges, between 29 and 40 mm. in length, 20 to 25 mm. in width, and between 7 and 10 mm. thick.

These scrapers are absent at other sites in the Northwest Territories but appear at the Little Arm site in the Yukon (Johnson, 1946, Pl. 22, *1*), the Campus (Rainey, 1939, Fig. 8, *15*), and Cape Denbigh sites (Giddings, 1951, Fig. 65, *2, 6-11*), in Alaska, and, as Nelson (1937) has pointed out, they also occur in outer Mongolia in the Neolithic and Mesolithic sites, as well as in Siberian sites of the same age (Sosnovsky, 1933, Fig. 3, *3-5*). This type was probably hafted and used for fleshing and scraping.

*Keeled End Scraper.* (Fig. 67, 6). The 11 end scrapers of this type are made from large crude lamellar flakes and thus have concave ventral surfaces and a single ridge on their dorsal sides. One end is rounded and steep, showing retouching, while the sides taper to a pointed end or squared end. They range from 35 to 54 mm. in length, 13 to 22 mm. in width, and 4 to 8 mm. in thickness. They were probably hafted.

This type of scraper, often called a convex end of the blade scraper, characterizes the upper Paleolithic of Europe though it is fairly abundant in the Mesolithic. It appears in the Lake Baikal region (Okladnikov, 1950, and Field and Prostov, 1937, Fig. 1, 5), and in Mongolia (Maringer, 1950, Pl. 27, *1-7*), in the Mesolithic and Neolithic. In northwest North America it appears at the Cape Denbigh site (Giddings, 1951, Fig. 59, *b*, 6-7), at the Campus site (Nelson, 1937, Fig. 15, *b*), at site at Milepost 422, British Columbia (Johnson, 1946, Pl. 23, 5), and at Natalkuz Lake in British Columbia. In the Northwest Territories it occurs at the Fishermans Lake site, the Franklin Docks site, and in sites belonging to the Lockhart River complex (MacNeish, 1951, Pl. 6, 8).

*Flat End Scraper* (Fig. 67, *11*). This single artifact has a very slightly convex bottom and a slightly more convex dorsal surface. It is only 2 mm. thick. One end is round and has fine retouching on it. It is 14 mm. wide and its length cannot be determined since it is broken. Scrapers like these appear in late prehistoric and historic Slavé sites and one occurred at Cape Denbigh. It probably functioned as a knife rather than as a scraping tool.

*Single Edge Flake Side Scraper* (Fig. 67, *12*). This type is composed of 37 thin flakes (never more than 5 mm. thick) of various shapes. Ventral surfaces are usually straight or concave and dorsal edges bear retouching along one of the longer edges.

These tools were probably used for cutting (as probably were unretouched flakes) and have widespread affiliations.

*Double Edged Flake Scrapers* (Fig. 67, *13*). All 15 of these have flat concave ventral surfaces and dorsal surfaces and are relatively flat except for retouching

along two adjacent sides. Though there is considerable range of form, most of them (7) are long and have retouching along two edges coming to a point.

These probably were cutting tools and are rare in the Northwest Territories. They are not described as occurring at other sites in the North, but I should not be surprised to find that they occur at many locations.

*Thick Pointed Side Scrapers* (Fig. 67, *16*). These 3 portions of artifacts are ventrally flat but dorsally roughly keeled. There is retouching along their dorsal edges leading to their points and percussion flaking up to their keeled backs. All three are about 18 mm. thick and 50 mm. wide.

They may have been side scrapers or part of knife blades and the only analogous implements to them are found at the Campus site in Alaska (Rainey, Fig. 7, 5-6), and at sites in the Trans-Baikal of Siberia (Sosnovsky, 1933, Fig. 3, *10*).

*Double-edged Plano-convex Side Scraper* (Fig. 67, *15*). These 14 specimens have flat or slightly convex bottoms and concave tops. They are usually roughly oblong, ranging from 20 to 75 mm. in length, 19 to 50 mm. in width, and 7 to 14 mm. in thickness, and have retouching on their two longer sides. They were probably cutting tools and are, as far as I can find out, unique to Pointed Mountain.

*Large Flake Side Scrapers*. There are 13 of these large irregular fragments of flint with retouching along one edge adjacent to a fairly flat surface. They are really not a definable type and do not seem to have a significant distribution.

*Prismatic Flake Side Scrapers* (Fig. 67, *14*). Seven large prismatic side scrapers have retouching along one of their edges. The retouching is fine and may be due more to wear than to conscious manufacture.

Similar tools are found at the Franklin Tanks site in the Northwest Territories and they are known from the Campus site and in the Trans-Baikal.

*Bifaces*. There are 32 of a general class of large artifacts I have called bifaces. There is a variety of forms, uses, and functions but all are bifacially chipped, and are considered together.

*Ovoid Blades* (Fig. 67, *17, 19*). The 10 fragments and 2 complete blades are from 30 to 66 mm. in width and about 11 mm. in thickness. The whole specimens are 98 and 46 mm. long. They are all ovoid with blunt points. Rude percussion flaking appears on both surfaces and they are retouched along their edges. They were probably an all-purpose tool used for cutting, piercing, scraping, etc. (though my workman suggested they were blades of daggers). They are a widespread type in the north and evidently belong to a number of time periods.

*Square-based Blades* (Fig. 67, *18*). There are 2 basal fragments of this type. One is crude, being 13 mm. thick and having a maximum width at its base of 45 mm. It bears rude chipping on both its faces and retouching at its edges. The other is better made, having a maximum thickness of 5 mm., and a width of 36 mm. Chipping on both surfaces is well done, and retouching along the edges. In the north, types such as these seem restricted to the Pointed Mountain site and may very well have been knife or dagger blades.

*Choppers*. This type is composed of a series of 16 thick bifaces (15 to 42 mm. thick) often made from pebbles. Their surfaces usually bear rough percussion flaking scars and one or more edges show some retouching or a series of wear flakes. All these tools seemed to have been used for chopping, though some may be quarry blanks. They are a widespread type.

*Chipped Pebble Hammerstones*. The 4 hammers are made from pebbles and are more or less spherical with percussion flaking on all surfaces. One small section usually shows battering as if used in hammering. These tools are restricted to the Pointed Mountain site.

Comparisons with Pointed Mountain may be divided into two general categories: comparisons of Pointed Mountain traits with those from sites in the same region (here called the Simpson-Liard area), and comparisons with sites yielding a somewhat similar artifact complex from more distant areas.

In the Simpson-Liard area of the Northwest Territories artifacts have been recovered from 18 localities. During the survey I was told of as many more sites but have not yet been able to visit them. Heavy vegetation made test pits necessary on most of the 18 sites discovered; none of them yielded large artifact inventories. However, from what has been uncovered, it is possible to delineate tentatively three cultural complexes different from Pointed Mountain that probably belong to different time periods.

The Sandy Lake complex is known from two components. One component, N.W.T.40, was situated on a high terrace of the Black River where the trail goes westward to Sandy Lake. Beside chips, a single lanceolate point (about 65 mm. long and 19 mm. wide) with a concave base, ground basal edges, and rough oblique ripple flaking of gray (Keele River) obsidian occurred (Fig. 68, *c*, 3). The other site was at P. Betrand's cabin on a high beach at the west end of Sandy Lake (Maxhameish) Lake. Excavation uncovered 29 artifacts in the beach sands below the humus. One projectile point which was 90 mm. long, with a maximum width at the shoulders of 23 mm. and 5 mm. thick. It is roughly diamond-shaped in cross-

section, has long tapering slightly convex edges, a straight base that is ground, and a stem 12 mm. long, with ground edges, which is only slightly narrower than the shoulders (20 mm.). One surface has rough collateral chipping, while the other displays more irregular chipping. Fine retouching is found along its edges. It looks very much like a crude Scottsbluff Point (Fig. 68, *c 1*). The other point is lanceolate in outline (72 mm. long with a maximum width near the midpoint of 28 mm.) with a narrow concave base. The surfaces are irregularly chipped, the edges retouched. The edges at the base and along the lower sides up to 20 mm. from the base are ground. This point, like the one at N.W.T.40, resembles the Long points (Fig. 68, *c, 2*). Blade fragments of three other points were found and one is of interest as it has a large flute extending almost to its point (Fig. 68, *c, 4*). Parts of five snub-nosed end-scrapers with flat tops (Fig. 68, *b, 7*), an ovoid blade, and parts of flake side-scrapers occurred (Fig. 68, *b, 8, 9, 10*).

From what we know of the Sandy Lake complex, it bears little or no resemblance to that from the Pointed Mountain site. The projectile points indicate that it might be related to the Great Bear River site, sites in the Yukon, and sites in the Plains further south. Its temporal relation to Pointed Mountain is equally difficult to discern. Like the Pointed Mountain site, the materials occur under the humus (and therefore before the advance of the forests) so it would seem that it has some antiquity. Moreover, on the west end of Great Bear Lake at the Bear River site (N.W.T.53) 6 Long points and 83 other artifacts have been found under a stratum containing a cultural component with microliths and other artifacts similar to those at Pointed Mountain. For this reason I believe we may tentatively consider the Sandy Lake complex to be earlier than Pointed Mountain.

The Fishermans Lake complex is composed of materials from 7 sites. One of these, site N.W.T.41, partially excavated during the four days training period prior to the excavation of the Pointed Mountain site, is situated on the northwest end of Fishermans Lake 2 miles from Pointed Mountain. It is on a lake terrace about 30 feet above the lake level and excavation of 27 five-foot squares revealed that artifacts occurred in the lowest part of the humus above pale brown loam (the $A^2$ horizon). Since artifacts of this locale are in a soil zone later than those of the Pointed Mountain site, and since the site is on a lower terrace, it seems safe to conclude that it is more recent than the Pointed Mountain complex.

Twelve of the 16 artifact types from the Fishermans Lake site occur at the Pointed Mountain site. The 6 unretouched lamellar flakes, the lamellar flake with two edges retouched (Fig. 68, *b, 5*), the 2 keeled end-scrapers, the 2 prismatic flake side-scrapers (Fig. 68, *b, 4*), one long side-notched point (Fig. 68, *b, 3*), and the 2 peaked end-scrapers (Fig. 68, *b, 9*), are traits held in common between the two complexes that seem indicative of some sort of genetic connection. Three large teardrop shaped end-scrapers (Fig. 68, *b, 8*), 5 thin flake side-scrapers retouched on one side (Fig. 68, *b, 6*), 8 large crude flake side-scrapers (Fig. 68, *b, 12*), small ovoid blades, large ovoid blades, and choppers also occur at both sites but are of such a generalized nature that it is difficult to say whether they represent nexus or not. Two long or short side-notched points with deep concave bases (eared points) (Fig. 68, *b, 1, 2*), one rectangular end-scraper (Fig. 68, *b, 7, 10*), 2 hoe-like scrapers (Fig. 68, *b, 13*), and 3 thin flakes with very delicate steep retouching along one concave edge (Fig. 68, *b, 11*), called Yukon knives, occur only at the Fishermans Lake complex while burins, large number of microlithic types and contracting stemmed points, in all 19 traits, occur at Pointed Mountain but not at N.W.T.41. In summary, the comparison seemed to indicate that though there are hints of some sort of genetic connection from the Pointed Mountain complex to the Fishermans Lake complex, but these two complexes are in reality very different. These significant differences may be in part due to the fact that they are widely separated in time.

The final grouping of artifacts is called the Spence River complex. Probably a number of sites I visited belong to this complex, but only two of those tested produced a large sample of materials: the Spence River site (N.W.T.47) at the junction of the Spence and Mackenzie rivers and J. Klondike's site (N.W.T.63) on a low terrace 10 feet above water level on the northwest shore of Fishermans Lake about 200 yards north of N.W.T.41. Traits that are distinctive of the complex are: small side-notched points (Fig. 68, *a, 1, 3*), small corner-notched

(Fig. 68, *a*, *2*), small end-scrapers with a convex end and a concave side forming a spur on one side (Fig. 68, *a*, 8), and small plano-convex end-scrapers (Fig. 68, *a*, 6). Large hoe-like sandstone scrapers (Fig. 68, *a*, *11*), and rectangular end-scrapers (Fig. 68, *a*, *7*), occur in both the Spence River and Fishermans Lake complex, while ovoid blades (Fig. 68, *a*, *10*), keeled end-scrapers (Fig. 68, *a*, *4*), flat end-scraper with convex edges (Fig 68, *a*, *12*), large crude flake side scrapers (Fig. 68, *a*, 9), small flake side-scrapers (Fig 68, *a*, 5), and double-edged flake side-scrapers occur in the Pointed Mountain, Fishermans Lake, and Spence River congeries. While there are many specific artifact differences between Pointed Mountain and Spence River, there does seem to be a thin thread of continuity from Pointed Mountain to Fishermans Lake to Spence River complexes.

The fact that the materials of the J. Klondike site were on the most recent beach around Fishermans Lake and the fact that the artifacts were uncovered from the top of the humus covered only by an inch or two of moss, indicate that the Spence River complex is the most recent one found so far in the Simpson-Liard area. Furthermore the occurrence of the hoe-like sandstone scraper in the Spence River complex identical with that used until very recently by the Slavé, the fact that bone and even fish scales were found preserved at N.W.T.63 in an area where acid soil causes rapid deterioration, and that a square-headed nail and part of a reworked iron file occurred with the artifacts at N.W.T.63, lead me to the conclusion that the Spence River complex probably represents part of the material of the late prehistoric or early historic Slavé or Nahani.

Thus from a study of the artifacts of the Simpson-Liard area it is possible to suggest that the Spence River complex is the youngest, preceded by Fishermans Lake complex, the Pointed Mountain complex, with Sandy Lake complex the earliest.

Comparative studies of these various trait complexes reveal no evidence of a genetic connection between the Sandy Lake and Pointed Mountain complexes. However, between Pointed Mountain, Fishermans Lake and Spence River, though there are many differences as to artifact types and proportions of artifact types, there does seem to be a tenuous thread of continuity and hints of a genetic connection from a microlithic industry to a historic Athabascan tribe. This hint or hypothesis should be further tested by work in that area.

Comparisons of the Pointed Mountain complex with similar cultures outside the Simpson-Liard area are most intriguing but any relationships indicated by similar trait complexes must be tentative because of the great distances between excavated sites. Northward from Pointed Mountain on the west end of Great Bear Lake a stratigraphic sequence has been uncovered. The earlier two cultures (the earliest having Plainview and Plainview-like points and the next having Long points) need not concern us because they show little or no resemblance to the artifacts from Pointed Mountain. However, the third culture of this sequence, found in the middle part of the present humus, is of interest. This culture, like that of Pointed Mountain, has unretouched lamellar flakes, lamellar flakes with one side retouched, lamellar flakes with two sides retouched, keeled end-scrapers, prismatic flake side-scrapers, flat end-scrapers, small triangular steep-sided end-scrapers, tear-drop shaped snub-nosed end-scrapers, and a side-notched point. Beside these significant traits in common, more general ones also occur such as: choppers, ovoid blades, chipped pebble hammers, square-based blades, thin flake side-scrapers retouched on one side, thin flake scraper retouched along two edges, and large crude flake scraper retouched along one side. Even though the burins, some of the more specialized microlithic types and some of the large bifacial types are missing from the materials in the upper levels of Franklin Tanks site on Great Bear Lake, these 17 traits held in common, 9 of which are very specialized, seem to indicate some sort of genetic relationship. Even more important than this nexus is the fact that a microlith industry in the north occurs on top of, and later than, components with early man types of materials.

The Campus site seems to be the site most closely related to the one at Pointed Mountain and a host of specialized traits occur at both. The two sites have in common both types of polyhedral cores (sub-conical and tongue-shaped), lamellar flakes, lamellar flakes with retouched rounded ends, lamellar flakes with retouching along one side, lamellar flakes with retouching along two sides, pointed lamellar flakes with retouched rounded ends, angled and corner burins, contracting stemmed projec-

tile points, long side-notched projectile points, tear-drop shaped snub-nosed end-scrapers, small triangular end-scrapers, keeled end-scrapers, prismatic flake side-scrapers, and large, rough pointed side-scrapers. The two sites, also, have a number of more general traits such as: ovoid blades, large rough end-scrapers, large plano-convex side-scrapers, small flake side-scrapers, and choppers. Traits not present at the Alaska site but occurring at N.W.T.42 are: micro-gouges, chipped pebble hammers, micro-drills, notched lamellar flakes, and some of the more specialized retouched lamellar flakes. All of these are numerically rare at Pointed Mountain as are the semi-lunar side-scrapers and rectangular end-scrapers that appear only at the Campus site. From the above evidence, it seems fairly clear that the Pointed Mountain and Campus sites are closely related. However, definite determination of the relationship and understanding of what it means must await the excavation of more inland sites from Alaska and the Yukon with large inventories that are adequately dated.

The undated Birch Lake site from central Alaska (Skarland and Giddings, 1948), the mixed Dixthada site in the upper Tanana (Rainey, 1939), and sites with microliths from the Yukon (Johnson, 1946) dated as being in the postglacial optimum from 4,000 to 7,000 years ago have yielded very few artifacts. Most of the artifacts from each of these sites are like those from Pointed Mountain indicating some sort of relationship, but in each case the site inventories are so small that it is difficult to say anything definite. They are but tantalizing hints that may eventually lead to the solution of the problems connected with the microliths at Pointed Mountain.

Before passing on to other more distant sites, the Natalkuz Lake site from central British Columbia (I examined the specimens through the courtesy of Carl Borden) should be mentioned. Here Borden, in 1951, found lamellar flakes, sub-conoidal polyhedral cores, keeled end-scrapers, keeled side-scrapers, neatly chipped ovoid blades associated with a series of large crude blades, choppers, end- and side-scrapers in a large circular pit house (35 feet in diameter) underneath late prehistoric Carrier implements. In a general way this complex resembles those mentioned above but there are many specific differences.

Besides these northern interior sites there is a series of Arctic sites that somewhat resemble the Pointed Mountain complex, such as: The west Greenland Paleo-Eskimo culture (Solberg, 1907; Meldgaard, 1952), Dorset from northeast Canada (Collins, 1953), and the Cape Denbigh culture of the Stewart Peninsula of Alaska. These cultures have in common with Pointed Mountain polyhedral cores, lamellar flakes, corner burins, angled burins, small triangular end-scrapers, flake side-scrapers with one edge retouched, flake side-scrapers with two edges retouched, and ovoid blades. Denbigh has beside these traits, end-of-the-blade scrapers, lamellar flakes with retouched rounded ends, pointed lamellar flakes, and lamellar flakes with retouching on one or both sides like those of Pointed Mountain. Among the specialized traits in these Arctic cultures not at Pointed Mountain are side and end blades or bladelets and triangular blades, while the interior cultures have heavy tool complexes which seems to be absent on the Arctic coast. Thus though there is some sort of relationship indicated by similar traits at Pointed Mountain and the Arctic sites, it is not so close as between Pointed Mountain and certain northwest interior sites.

One cannot but speculate that both the interior and coastal sites may have sprung from a common ancestral type of culture, but that the interior sites (with the biface forms) have undergone some specialization toward forest living, while the early Arctic cultures (with side blades, end blades, and triangular points and few, if any bifaces) have become specialized toward living in the Arctic coastal environment.

Any discussion of the origin of industries of North America that have microliths leads to a consideration of similar materials in northeast Asia. The data now available from northeast Asia are for the most part undated and what has been described or illustrated is not from near Bering Strait but in the Trans-Baikal region of Siberia or Mongolia. Such materials are too distant and the complex too poorly defined to indicate any sort of concrete New World to Old World nexus. In a general way, some of the artifact complexes from the northeast Asiatic interior are like some of those from the northwest American interior, as many of the sites from both areas have choppers, large scrapers, snub-nosed end-scrapers, Aurignacian-

like burins, and big blades (Paleolithic tools) in association with microliths (Mesolithic tools) (Beregovaya, 1948). As has been pointed out previously, there is a host of artifacts in the Pointed Mountain assemblage that have their counterparts in the Trans-Baikal region or outer Mongolia. These include: sub-conoidal polyhedral cores, tongue-shaped polyhedral cores, unretouched lamellar flakes, lamellar flakes with retouched rounded ends, pointed lamellar flakes with work along their edges, lamellar flakes with one or two edges retouched, notched lamellar flakes, lamellar flakes with retouched square ends, beaked lamellar flakes, micro-burins, larger corner burins, angle burins, lamellar flake drills, ovoid blades or projectile points, large plano-convex end- or side-scrapers, various kinds of triangular snub-nosed end-scrapers, end-of-the-blade scrapers, various kinds of thin flake side-scrapers, large ovoid blades, and choppers. This is rather a long list but all it means at present is that there is a good hint of connection between northeast Asia and northwest America. Only by the finding of a complex of traits such as listed above at a series of dated sites from Lake Baikal across northeastern Siberia and across Alaska can we demonstrate evidence of migration. In light of Okladnikov's statement to the effect that he found implements very similar to those from the Alaskan Campus site at sites on the Kolyma River and that these Siberian sites were in reddish (leached?) loess like the Campus site of Alaska, the Kluane Lake site and Pointed Mountain site, indicating a possible temporal correlation, it is not unreasonable to expect that eventually the necessary evidence for such Asiatic-American connection will be uncovered. However, all we have so far is guesses, and it is hoped with this new material, better guesses than we have had previously.

## Summary

The Pointed Mountain site, near Fort Liard in the southwest corner of the Northwest Territories of Canada, was excavated in 1952. At this site a series of micro-blade tools made from specialized polyhedral cores was found in association with a number of types of scrapers, projectile points, and other bifaced tools. This artifact complex was found in loess and may be dated as from 5,000 to 8,000 years old. The excavation and analysis of this artifact complex in no way solves the many problems connected with early migrants into the New World but these materials may serve as evidence that may be brought to bear upon some of these problems.

Comparisons with the Pointed Mountain site data do indicate, however, some sort of tentative conclusions and predicate further lines of research. In the Fort Liard area it would seem that there is at least a sequence of four cultural complexes from early to late, Sandy Lake, Pointed Mountain, Fishermans Lake, and Spence River. Furthermore, there are hints of a thread of cultural continuity from the Pointed Mountain microlithic assemblage to the Spence River complex which may be connected with a historic Athabascan culture.

On a more general level, it is suggested that in interior northwest North America (i.e., central British Columbia, the southern Yukon, and the eastern part of the Northwest Territories of Canada, as well as central Alaska) that there is a series of related sites with a distinctive cultural pattern. This pattern is characterized by the presence of polyhedral cores (often of the tongue-shaped type), retouched and unretouched lamellar flakes, and end- and side-scrapers made from large prismatic flakes, in association with snub-nosed end-scrapers, thin and thick crude side-scrapers, rough end-scrapers, bifacial blades, choppers, and large projectile points. On the basis of the finds at Pointed Mountain and the re-examination of the Campus site materials, it is possible that crude corner and angled burins (like the Aurignacian types of Europe) and types of retouched lamellar flakes are also a feature of this pattern. Since some of the sites (Kluane Lake sites and Pointed Mountain) with this pattern seem to be fairly early, 4,000 to 8,000 years ago, it may be possible that these assemblages began or existed at such an early date.

This early interior northwest North American cultural pattern seems to be distantly related to another early pattern along the Arctic coast, often called the Paleo-Eskimo. It, however, differs from it in that the interior Northwest assemblages have a series of heavy tools while these others have not, and in that the early Arctic coastal sites have a series of distinctive tools such as side blades, end blades, and triangular points. Both of these patterns may have developed from some as yet undefined early (Mesolithic) Paleo-Siberian cultural complex.

## BIBLIOGRAPHY


Auerbach, N. K. and G. P. Sosnovsky

1932. *Contributions to the Knowledge of the Paleolithic Industry and the Contributions of its Occurrence in the Afontova Gora Site.* Trudy, Leningrad. S.S.S.R.

Beregovaya, N.

1948. Dreoneishie Kul'tury Alyaski i Vopros o Zaselenii Ameriki. *Sovetskaya Etnografiya*, No. 4, S.S.S.R.

Burkitt, M. C.

1921. *Prehistory.* Cambridge, England.

Clark, S. G. D.

1936. *The Mesolithic Settlement of Northern Europe.* Cambridge, England.

Collins, Henry B.

1953. Recent Developments in the Dorset Culture Area. In "Memoirs of the Society for American Archaeology, Memoir 9," Salt Lake City, Utah.

Field, Henry and Eugene Prostov

1937. Archaeology in the Soviet Union. *American Anthropologist*, Vol. 39, No. 3. Menasha.

Giddings, J. L.

1951. The Denbigh Flint Complex. *American Antiquity*, Vol. 16, No. 3. Salt Lake City.

Hage, C. O.

1945. Geological Reconnaissance Along Lower Liard, Northwest Territories, Yukon, and British Columbia. *Geological Survey Paper 45.* Ottawa, Canada.

Harp, Elmer

1951. An Archaeological Survey in the Strait of Belle Isle Area. *American Antiquity*, Vol. 16, No. 3. Salt Lake City.

Irving, William

1951. Archaeology in the Brooks Range. *American Antiquity*, Vol. 17, No. 1. Salt Lake City.

Johnson, Frederick

1946. An Archaeological Survey Along the Alaska Highway, 1944. *American Antiquity*, Vol. 11, No. 3. Menasha.

Krader, Lawrence

1952. Neolithic Find in the Chukchi Peninsula. *American Antiquity*, Vol. 17, No. 3. Salt Lake City.

MacNeish, Richard S.

1951. An Archaeological Reconnaissance in the Northwest Territories. *Annual Report of the National Museum of Canada, 1949-1950, Bulletin* 123. Ottawa.

1953. Archaeological Reconnaissance in the Mackenzie River Drainage. *Annual Report of the National Museum of Canada, 1951-1952, Bulletin* 128. Ottawa.

Maringer, John

1950. Contributions to the Prehistory of Mongolia. *Sino-Swedish Expedition*, Pub. 34, No. 7. Stockholm.

Meldgaard, Jorgen

1952. A Paleo-Eskimo Culture in Western Greenland. *American Antiquity*, Vol. 17, No. 3. Salt Lake City.

Nelson, N. C.

1926. The Dune Dwellers of the Gobi. *Natural History*, Vol. 26. New York.

1937. Notes on Cultural Relations between Asia and America. *American Antiquity*, Vol. 2, No. 4. Menasha.

Okladnikov, A. P.

1950. Lenskie Drevnosti. Materialyps Drevnei Istorii Iakutii. Institut Istorii Material'noi Kul'tury im N. Ia Marra, *Akademii Nauk*, S.S.S.R. Vol. 3.

Rainey, Froelich G.

1939. Archaeology in Central Alaska. *Anthropological Papers, American Museum of Natural History*, Vol. 36, Pt. 4. New York.

Raup, Hugh M.

1947. The Botany of Southwest Mackenzie. *Sargentia*, No. 6. Jamaica Plain, Mass.

Skarland, Ivar and J. L. Giddings, Jr.

1948. Flint Stations in Central Alaska. *American Antiquity*, Vol. 16, No. 2. Menasha.

Solberg, O.

1907. *Beitrage zuy Vorgeschichte der Osteskimo.* Christiania, Denmark.

Solecki, Ralph S.

1951. Notes on Two Archaeological Discoveries in Northern Alaska, 1950. *American Antiquity*, Vol. 18, No. 1. Salt Lake City.

Sosnovsky, G. P.

1933. *Traces of Paleolithic Man in the Trans-Baikal.* Trudy, Leningrad, S.S.S.R.


National Museum of Canada,
Ottawa, Ontario.
April, 1953.

# OBSERVATIONS ON THE BUTCHERING TECHNIQUE OF SOME ABORIGINAL PEOPLES NOS. 3, 4, 5, AND 6 *

THEODORE E. WHITE

## NO. 3: SMALL vs. LARGE ANIMALS

THIS STUDY is based on the bison, deer, and antelope bone from a fortified earth lodge village (32ME15) in the Garrison Reservoir Area. The site was excavated by River Basin Survey parties in 1950 and 1951, under the supervision of G. Ellis Burcaw and Donald D. Hartle respectively. The animal bone from this site is of particular interest because it permits a comparison to be made between the methods used on the smaller food animals and those used on the bison. In a previous study an attempt was made to compare the methods used on antelope and on bison, but the methods were those of very different peoples who were widely separated both geographically and temporally and the differences could as easily have been due to culture as well as the size of the game. Since this material is from a single component site, any difference in technique can safely be attributed to the size of the game.

Since this study is primarily concerned with possible differences, due to size, in the method of cutting up the carcass of the food animal, the deer and antelope bone have been lumped together in order to obtain a larger sample in this size range. Although deer average a little larger than antelope the difference is small enough that for this study it is negligible. In fact, the weight of a large antelope often exceeds the average for deer.

The numerical count of the elements found in a site is subject to the accidents of preservation and the size of the excavation, but the ratio of the various elements to each other and to the greatest number of individuals represented, from an excavation which meets the archaeological requirements, should provide an adequate sample for this type of study. Whenever possible both the worked and unworked bone have been taken into consideration, though the former are often unidentifiable. In calculating the number of individuals represented by any element the number of that element was tabulated for both right and left sides and the greater taken. In calculating the number of individuals represented by the vertebrae the total number of vertebrae for a particular region is divided by the appropriate number for that region; i.e., the total number of dorsal vertebrae of bison was 133; this was divided by 13 which gives a minimum of 11 individuals represented. The percentages given are calculated on the basis of the greatest number of individuals represented by any element.

The distribution of the various elements from this site presents a number of differences, presumably of cultural significance, when compared with those which formed the basis of the two previous studies. However, with so few studies of this sort it would be premature to attempt to evaluate these differences.

Judging by the habits of Indians living in this area within historic times, there were two sources of bison meat for these people; the hunt and those which drowned in the river by falling through the ice. That these people utilized both sources is only conjecture because the preservation of the bone does not permit any such grouping.

The limited literature on butchering technique (Wilson on the Hidatsa, and Wissler on the Blackfeet) confirms some of the inferences drawn and these will be noted under the consideration of the particular element. From both of these accounts it appears that normally the men do the hunting and gross butchering and the women the final cutting up of the meat for smoking and drying. Both helped carry the meat from the kill to the hunting camp or village.

*Skull.* The relatively small percentage of skulls in this site indicates that it does not enter very greatly into these peoples' economy. How-

* EDITOR'S NOTE. These four papers, published by permission of the Secretary of the Smithsonian Institution, were received during the period March through June, 1953, and are here grouped as one article for convenience. Paper Nos. 1 and 2 have already appeared in *American Antiquity*, Vol. 17, No. 4, and Vol. 19, No. 2, respectively.

ever, a greater proportion of the horns and occiput of the smaller game than of the bison were brought into the village. This appears to be correlated with the size of the game since the brain could conveniently be removed at the kill. If killed reasonably close it is conceivable that the head of the small game may have been brought in for further cutting up.

Of interest here is the fact that the percentage of maxillae greatly exceeds that of either the occiput or horn cores. Were the nasal cartilages considered a delicacy? Or desired as a base for broth?

*Mandible.* With both the large and small game the number of mandibles is very close to the total number of individuals represented. Presumably these were brought in with the tongue as none of them show evidence of having been worked. Certainly the quickest and easiest way to remove the tongue would be to smash the ascending ramus of the jaw and remove both tongue and jaw as a unit. A pair of heavy bison jaws would unnecessarily increase the weight of the load to be brought in; consequently it is inferred that the jaws represented the animals killed close to the village or retrieved from the river. As far as the ethnographic data indicate, the tongue was considered a choice cut and was eaten within a few hours after the kill (Wilson, 1924, p. 249).

*Hyoid.* Very few hyoids were recovered from this site. Normally they would be removed with the tongue and brought into the village. However, they are a light bone and might have been eaten by dogs or otherwise broken up and lost.

*Vertebra.* With both the large and small game a very small percentage of the individuals are represented by the vertebrae. The inference to be drawn from this is that the axial muscles were cut away and the vertebra left at the kill. According to the ethnographic data (Wilson, 1924, p. 201) the rejected pieces were sometimes gathered up to feed the dogs. After the hunter had taken what he wanted that which was left became common property and was often picked up by others for their dogs. The inference then is that the vertebrae which are found in the village are those which were brought in for dog food. The very small number of caudal vertebra indicates that the tail was neither in general use as an ornament nor left with the hide after skinning.

*Ribs.* A count of the ribs was not made but it is estimated that they make up between 50 and 60 per cent of the gross count of bone. On the other hand rib heads are very scarce, indicating that the ribs were chopped loose from the vertebral column and the side brought in as a unit.

*Forelimb.* Since neither the large or small food animals possess a clavicle the foreleg could be removed in its entirety and brought to the village or hunting camp for further cutting. Since the number of scapulae of the smaller game is close to the total number of individuals represented it indicates that such was probably the case with them. But with the bison scapulae we find the greatest difference in the methods used on large and small game (and also very different from the other sites studied). The bison scapulae recovered from this site represent only 45.7 per cent of the total number of individuals, and only 20 per cent of the scapulae were converted into hoes. There are two inferences to be drawn from these data. First, this area was too far north for primitive agriculture to be profitably pursued. Second, it is relatively easy to strip the meat from a scapula and discard the bone. Presumably this was the procedure unless the animal was killed very close to the village or retrieved from the river. If the meat had to be transported any distance the removal of the scapula would materially lighten the load in the case of the bison, but would be a negligible quantity in the case of the deer or antelope. Also, the marrow content of the scapula is so low that there would be no point in bringing it in except for utilitarian purposes.

Although the proximal end of the humerus of the bison represents a very low percentage of the individuals, the distal end shows a higher percentage than any of the other elements of the foreleg. Not only does the humerus carry more meat but also has a higher marrow content than the radius and ulna. Presumably, then it was generally brought into the village for both purposes. The small percentage of the proximal end of the humerus can be partially explained in that they were converted into hide-burnishers (Wilson, 1924, p. 283) and worn out. Since the number of the radius and ulna agree quite well with the scapula, it is assumed that they represent those individuals killed very close to the village or retrieved from the river.

With the exception of the scapula the elements of the forelimb of the small game are more or less in accord with those of the bison.

Since their size will not seriously affect the load to be brought to the village, there are two other possibilities which must be considered to account for the low percentages. These elements are small, light bones and could either have been smashed beyond recognition in the preparation of bone grease (Leechman, 1951, p. 355), or have been eaten by dogs after cooking.

*Hind leg.* In order to remove the hind leg from the trunk it is necessary to cut through bone. This appears to have been accomplished in one of two ways; to cut the ilium just back of the sacral attachment and split the pelvis at the symphysis, or to cut through the neck of the femur, thus freeing the shaft from the pelvis. After the bone had been exposed a single blow with an ax would have sufficed for either method. Both methods appear to have been used but from the relative scarcity of the pelves (7 per cent for the bison and 30 per cent for the smaller game), it would appear that severing the neck of the femur was the commoner method. The size of the game seems the most logical explanation for the difference in the percentages. In the case of the bison the weight of the pelvis would materially increase the load to be transported, while with the smaller game the weight would be negligible.

As far as the ethnographic data goes (Wilson, 1924, p. 228 and 235), we are permitted to assume that the hind leg above the tarsus was brought back to the village or hunting camp. The femur (ham bone) appeared to be particularly in demand for the marrow (Wilson, p. 249). The low percentages of all except the distal end of the tibia appears to be explainable on the grounds that they were smashed beyond recognition in the preparation of bone grease. That the distal end of the tibia is preserved may be because it formed a convenient handhold for steadying the femur and tibia on the anvil stone while it was being smashed with a hammer stone. The low percentages of the femur and the proximal end of the tibia of the small game can be at least partially explained in that they were eaten by the dogs after being cooked, as they are small light bones.

*Metapodials.* According to Wilson (1924, p. 251, 202) the metapodials were usually discarded or given to the dogs. The percentage of metapodials of the small game is considerably higher than that of the bison. Since the bison metapodials would materially increase the load, it would appear that the distance from the village would be the deciding factor on whether or not they were brought in. The additional weight of the metapodials would not be as important a factor in the case of the smaller game.

TABLE 10. FREQUENCY DISTRIBUTION OF THE BISON AND DEER-ANTELOPE BONE FROM 32ME15

| | Bison | | Deer and Antelope | |
|---|---|---|---|---|
| Element | Indivs. Repres. | % of Total | Indivs. Repres. | % of Total |
| Skull, occiput | 26 | 15.8 | 7 | 20 |
| " , horn cores | 24 | 15.6 | 7 | 20 |
| " , maxilla | 62 | 37 | 13 | 38 |
| Mandible | 158 | 96 | 33 | 97 |
| Hyoid | 13 | 8 | 1 | 3 |
| Vertebra, atlas | 23 | 15 | 2 | 6 |
| " , axis | 18 | 11 | 2 | 6 |
| " , cervical | 10 | 6 | 2 | 6 |
| " , dorsal | 11 | 7 | 1 | 3 |
| " , lumbar | 13 | 8 | 3 | 9 |
| " , sacral | 13 | 8 | 1 | 3 |
| " , caudal | 1 | --- | --- | --- |
| Scapula | 75 | 45.7 | 31 | 91 |
| Humerus, proximal | 14 | 8.5 | 6 | 18 |
| " , distal | 121 | 77 | 14 | 41 |
| Radius, proximal | 75 | 45.7 | 17 | 50 |
| " , distal | 72 | 44 | 12 | 35 |
| Ulna, proximal | 67 | 40 | 8 | 23 |
| Metacarpal, proximal | 37 | 22.5 | 16 | 47 |
| Pelvis | 12 | 7 | 10 | 30 |
| Femur, head | 36 | 22 | 1 | 3 |
| " , proximal | 42 | 28.6 | 1 | 3 |
| " , distal | 29 | 18 | 2 | 6 |
| Tibia, proximal | 48 | 30 | 5 | 14.7 |
| " , distal | 164 | 100 | 34 | 100 |
| Astragalus | 30 | 18 | 19 | 56 |
| Metatarsal, proximal | 32 | 20 | 9 | 26.4 |

## SUMMARY

The methods of preparing the carcass of large and small animals for food appear to be essentially the same with only minor differences which, by inference, are correlated with the distance necessary to transport the meat. Because of their greater weight the skull, scapula, pelvis and metapodials of the bison were not transported as far as the corresponding elements in the deer and antelope and were left at the kill. The low percentage of some of the limb elements of deer and antelope is partially explained in that they are small and light enough to have been eaten by dogs after cooking.

## NO. 4: COMPARISON OF BISON BONES FROM THREE EARTH LODGE VILLAGES

THE BISON BONE which provides the basis of this study was collected during the excavation of the Buffalo Pasture site, near Pierre, South Dakota. It was a single component site of relatively late occupation, with cultural affinities close to the Snake Butte focus (Lehmer, n.d.). The site (a fortified earth lodge village) was excavated by a River Basin Survey party during the summer of 1952 under the supervision of Franklin Fenenga. I have outlined the methods and procedures used in pursuing this type of study (White, 1952).

The principal value of the analysis of any unit of a biotic assemblage, whether a paleontological fauna or the bone from an archaeological site, is its comparison with analogous units which are closely adjacent in time, and in a consideration of the geographical relationship of the units.

The comparison to be made in this study is between the frequency distributions of the various skeletal elements of bison from the Phillips Ranch, Buffalo Pasture, and Rock Village sites. The Phillips Ranch and Buffalo Pasture sites are only about two miles apart. They lie about eight miles north of Pierre, South Dakota. Rock Village is located about 65 miles northwest (air line) of Bismarck, North Dakota, nearly three and one-half degrees of latitude north of the first two. All of the sites are located in the valley of the Missouri River on the first terrace above the flood plain and very close to the present channel of the river. The relation of the villages to the main channel during occupation has not been determined but judging by the present system of meander scrolls the position of the channel then and now could not have differed but a few hundred feet.

*Skull.* The Buffalo Pasture site is exceptional in that no example of the occipital portion of the skull has been found. In the Phillips Ranch site the occiput represents only 2.5 per cent of the greatest number of individuals represented by any one element, and in Rock Village it represents 15.8 per cent. In the present state of our knowledge we cannot be certain that the difference between Buffalo Pasture and Phillips Ranch is not due to an accident of sampling, but the difference between those two and Rock Village is too great to be explained in this manner. Consequently, we are obliged to infer that the difference represents a difference in either the butchering technique or ceremonial customs of the people.

In both the Phillips Ranch and Rock Village sites the horn-cores which had not been converted into tools had been crudely broken from the frontal region of the skull, but in the Buffalo Pasture site 60 per cent of the horn-cores had been carefully cut off near the base. This, together with the much greater percentage of horn-cores, sharply distinguishes Buffalo Pasture from the other two sites. In Rock Village the percentages of occiputs and horn-cores are equal, which is a characteristic that distinguishes it from the other two sites.

The Phillips Ranch and Buffalo Pasture sites are quite in harmony with each other in that the percentage of the maxillae is only half that of the horn-cores, but in Rock Village it is twice that of the horn-cores. This indicates a considerable difference, presumably in food habits, between the peoples of Rock Village and the other two sites. Without the correlation with data of the other materials from the sites, it is impossible to be certain of the significance of the increase in the percentage of maxilla from Phillips Ranch to Rock Village. Superficially, it suggests steps in changing food habits, but this can be true *only* if the peoples are related and the chronological order is Phillips Ranch — Buffalo Pasture — Rock Village. Otherwise it is only a coincidence.

*Mandible.* Presumably the lower jaws were brought in with the tongue, which was considered a choice cut and was eaten within a few hours after butchering (Wilson, 1924, p. 249). The pattern of the frequency distribution of the lower jaws from the three sites is very similar to that of the maxillae and may indicate a trend. Two possibilities present themselves. Either it indicates the distance away from the village at which most of the bison were killed, the fewer jaws indicating the greater distance. Or with the acquisition of horses it was necessary to bring the meat back to the village for processing in order to avoid roving bands of other peoples.

*Hyoid.* The hyoid, small and fragile, is not suited for this type of study.

*Atlas.* In the Phillips Ranch and Buffalo Pasture sites the percentage of the atlas is greatly in excess of that of the occiput, but in Rock Village the percentages of the atlas, occiput and horn-cores are nearly equal. This indicates a very marked difference between the customs of the people of Rock Village and the other two sites.

A possible explanation of the excess percentage of the atlas over the occiput in the Phillips Ranch and Buffalo Pasture sites is in the method of removing the brain, yet saving the frontals for the manufacture of frontal hoes. This can be done by smashing the back of the skull just above the condyles and the basioccipital just in front of the condyles. The atlas if brought in with the skull would be discarded but the basicranium would be smashed beyond recognition.

*Vertebra.* The percentage of the other vertebrae is consistently low for all three sites, indicating that they were normally left at the kill. The somewhat higher percentage of vertebra for Rock Village is partially due to the discovery of a refuse heap in the fortification ditch.

Usually the caudal vertebrae have the smallest percentage of any of the categories, but in the Buffalo Pasture site there were two groups of eight vertebrae and one group of eleven, indicating that with these people the tail may have been used as an ornament or brought in with the hide. In the bison bone which I have identified from sites in the Missouri Valley this is the first indication I have encountered that the tail may have been brought in entire.

*Scapula.* In both the Phillips Ranch and Buffalo Pasture sites the sum of the worked and unworked scapulae represents the greatest number of individuals of any of the elements. The inference drawn is that the bison killed close to the village supplied inadequate numbers of scapulae for agricultural and other needs and it was necessary to bring scapulae from the group hunt. In Rock Village the sum of the worked and unworked scapulae is less than half that of the distal end of the tibia, the most common element, indicating perhaps that these people did not practice agriculture as extensively as those of the other two sites. There are two possible explanations, or a combination of both, for this difference. First, Rock Village is too far north to obtain a high yield from any of the varieties of corn which we know today and we cannot be certain that there ever were hardier varieties. Second, the acquisition of horses may have made subsistence on a principally meat diet much easier.

*Forelimb.* With the exception of the proximal end of the humerus the elements of the forelimb above the carpus from Rock Village totals roughly three times that of either of the other two sites. Possibly, because of roving bands of other peoples, the people of Rock Village found it expedient to bring the limb bones to the village to be processed for "bone butter" while the others processed the bones at the hunting camp or not at all. Or, since the peoples of Phillips Ranch and Buffalo Pasture did considerable farming, the carbohydrates of corn and beans may have reduced the amount of fat necessary in their diet.

*Metacarpals.* In all three sites the percentage of metacarpals is consistently below that of the other elements of the forelimb. In the Phillips Ranch and Buffalo Pasture sites the percentage of metacarpals is compatible with the distal end of the radius, indicating that in these two sites the metacarpals represent the bison killed locally or retrieved from the river. But in Rock Village the percentage of metacarpals is only one-half that of the distal end of the radius and less than one-third that of the distal end of the humerus, giving a very significant difference between the customs of the peoples of Rock Village and the other two sites. Since the marrow cavity of the metacarpals is very small it would not be profitable to transport them any distance for "bone butter."

*Pelvis.* The percentage of the pelvis found is very low in all three sites, indicating that it was normally left at the kill. The difference between the three sites does not appear to have any particular significance. The greater percentage of pelves from Rock Village is partially explained in that many of them came from the refuse heap mentioned in a previous paragraph.

*Hind limb.* Aside from the percentage of the distal end of the tibia in Rock Village, the frequency distribution of the elements of the hind limb is entirely compatible with that of the forelimb and the inference is inescapable that the factors which brought about the frequency distribution of one are also applicable to the other. The customs of the people of Rock Village are very markedly set off from those of the other two sites by the fact that the percentage of the distal end of the tibia not

only represents the greatest number of individuals for that site but is more than three times that of either of the other two.

*Phalanx.* One feature which sets the customs of the people of Buffalo Pasture off from the other two sites is the number of smashed phalanges, nearly two-thirds of those recovered. Usually it was the first phalanx involved but occasionally the second. Most of the phalanges were broken across but a few were split lengthwise. According to their frequency distribution the significance is not apparent.

TABLE 11. FREQUENCY OF THE BISON BONE FROM PHILLIPS RANCH, BUFFALO PASTURE AND ROCK VILLAGE

| | Number of Individuals Represented | | | Per cent of Greatest Number | | |
|---|---|---|---|---|---|---|
| Element | Phillips Ranch | Buffalo Pasture | Rock Village | Phillips Ranch | Buffalo Pasture | Rock Village |
| Skull, occiput | 2 | — | 26 | 2.5 | — | 15.8 |
| " , horn-cores | 13 | 50 | 24 | 16.6 | 36 | 15.6 |
| " , maxilla | 5 | 25 | 62 | 6.4 | 18 | 37 |
| Mandible | 19 | 82 | 158 | 24.0 | 60 | 96 |
| Hyoid | 9 | 31 | 13 | 11.6 | 22.4 | 8 |
| Vertebra, atlas | 7 | 6 | 23 | 9.0 | 4.3 | 15 |
| " , axis | 2 | 5 | 18 | 2.5 | 3.6 | 11 |
| " , cervical | 4 | 2 | 10 | 5.0 | 1.4 | 6 |
| " , dorsal | 2 | 2 | 11 | 2.5 | 1.4 | 7 |
| " , lumbar | 1 | 1 | 13 | 1.2 | — | 8 |
| " , sacral | 2 | 2 | 13 | 2.5 | 1.4 | 8 |
| " , caudal | 1 | 5 | 1 | 1.2 | 3.6 | — |
| Scapula | 78 | 138 | 75 | 100.0 | 100 | 45.7 |
| Humerus, proximal | 15 | 34 | 14 | 20.0 | 24.6 | 8.5 |
| " , distal | 20 | 36 | 121 | 25.0 | 26 | 77 |
| Radius, proximal | 15 | 30 | 75 | 20 | 21.7 | 46 |
| " , distal | 11 | 20 | 72 | 14 | 14.5 | 44 |
| Ulna, proximal | 9 | 18 | 67 | 11.6 | 13.0 | 40 |
| Metacarpal, proximal | 12 | 21 | 37 | 15 | 14.5 | 22.5 |
| Pelvis | 4 | 6 | 12 | 5 | 4.3 | 7 |
| Femur, head | 6 | 11 | 36 | 7.7 | 8.0 | 22 |
| " , proximal | 3 | 8 | 42 | 3.8 | 6.0 | 28.6 |
| Femur | 6 | 9 | 29 | 7.7 | 6.4 | 18 |
| Tibia, proximal | 8 | 12 | 48 | 10.0 | 8.7 | 30 |
| " , distal | 23 | 43 | 164 | 30.0 | 31 | 100 |
| Astragalus | 10 | 31 | 30 | 12.0 | 22.4 | 18 |
| Metatarsal, proximal | 13 | 14 | 32 | 16.6 | 10.0 | 20 |

## CONCLUSIONS

It must be borne in mind that the conclusions expressed here are based entirely on the frequency distribution of the skeletal elements of bison from the three sites and have *not* been correlated with the conclusions based on the artifact materials from the sites. This lack of correlation has been deliberate on my part in order that the reliability of the frequency distribution of the skeletal elements as a means of expressing the degree of difference in the customs of peoples inhabiting different sites could be weighed against the differences expressed by other materials.

1. The frequency distribution of the bison bone from the Phillips Ranch and Buffalo Pasture sites describes essentially the same pattern, indicating that the subsistence patterns were closely similar. Minor differences are indicated by the greater percentage of horn-cores, maxillae, lower jaws, caudal vertebrae and smashed phalanges in the Buffalo Pasture site.

2. On the basis of the frequency distribution of the bison bone the inhabitants of Rock Village were a very different people from those of the other two sites and show affinities with the people of the Monroe and Anderson foci of the Dodd site.

3. If the people of Rock Village and the other two sites are related, the move from the vicinity of Pierre, South Dakota to the vicinity of Rock Village in North Dakota would necessitate a radical change in their mode of life because of climatic factors; at least, agriculture appears to have been less important at the latter village.

## NO. 5: A COMPARISON OF THE BISON BONE FROM THE DODD AND ROCK VILLAGE SITES *

THE BISON BONE which forms the basis for this study is from the Monroe and Anderson foci of the Dodd Site (39ST30) located about 6 miles north of Pierre, South Dakota and Rock Village which is about 65 miles (air line) northwest of Bismarck, North Dakota. Both sites are situated on the right bank of the Missouri River but are separated by about 3½ degrees of latitude. This distance places Rock Village in a latitude which is probably very near the northern limit of primitive agriculture for the plains and the climate would be an important factor in the people's economy.

At the Dodd site the Anderson focus was superimposed on the Monroe and both probably date from before A.D. 1500 (Lehmer,

* In the preparation of this manuscript many helpful suggestions and considerable ethnographic data was received from George Metcalf of the River Basin Surveys.

n.d.). Rock Village is a single component site and is tentatively dated from 1825 to 1837 (Hartle, verbal communication). Although there is a wide time gap (300 years) between Rock Village and the Monroe and Anderson foci, there is a striking similarity in the over-all pattern of the frequency distribution of the skeletal elements from the three villages. Yet between the three villages differences in the frequency distribution exist which are too great to be explained as accidents of preservation or sampling, and which differentiate the villages. The inference is drawn that these differences are an expression of the differences in the habits and customs of the peoples. Although this is only the second comparison of this sort to be made, and the merits of this type of study cannot be considered conclusive, there is the suggestion that such comparisons are a very sensitive method of detecting minor differences in the cultural attributes of closely related peoples.

Although circumstances made it impossible or impractical to completely excavate the sites a sufficient number of houses were excavated at each village to furnish a workable sample of bone. The number of houses excavated at each village are: Monroe — 4; Anderson — 7; and Rock Village — 13.

*Skull.* There is a rather wide variation in the per cent of the occipital portion of the skull represented in the different villages. Anderson focus shows the lowest per cent (5.5) and Rock Village the highest (15.8) with the Monroe focus occupying an intermediate position (10). The difference is further emphasized if the distribution of the occiput by houses in the different villages is considered: Anderson — 0.28; Monroe — 1.5; and Rock Village — 2.0 occiputs per house. In this respect the people of the Anderson focus are very sharply set off from the other two. In the Monroe focus and Rock Village the per cent of the horn-cores agrees with that of the occiput and greatly exceeds that in the Anderson focus. Here again the Monroe focus and Rock Village are in harmony and differ radically from the Anderson focus. The Monroe focus and Rock Village agree in that the per cent of the maxillae recovered is roughly twice that of either the occiput or horn-cores. The Anderson focus is sharply set off from the other two not only in having the greatest per cent of the maxillae but also in that it is nearly 8 times that of the occiput. The frequency distribution of the parts of the skull shows a very substantial difference between the people of the Anderson focus and the other two villages.

*Lower Jaw.* Presumably the lower jaws are brought in with the tongue. All villages agree in that the per cent of the lower jaws is very close to the greatest number of individuals represented. Although the Anderson focus shows the lowest per cent, the number of individuals from this focus is small enough that a slight difference in the numbers would make a big difference in the per cent. In the present state of our knowledge we cannot be certain that this difference is significant.

*Hyoid.* The hyoid is unsuited for this type of study.

*Atlas.* The Monroe focus and Rock Village agree in that the per cent of the atlas is very nearly the same as that of both the occiput and the horn-cores. The Anderson focus differs markedly from the other two in that the per cent of the atlas is nearly 3 times that of the occiput. The head could be most easily severed from the neck at the joint between the atlas and axis and the atlas would be left with the skull. Presumably then the number of atlases brought into the village should be equal to or very close to the number of heads brought in. What, then, did the people of the Anderson focus do with the occiput and horn-cores? There is no evidence that they manufactured frontal hoes or horn scoops. This difference together with the very high percentage of maxillae sets them off very sharply from the other two villages.

*Vertebra.* The percentage of the vertebrae is consistently low for all three villages, indicating that they were normally left at the place of butchering. The differences in the percentages between the three villages are small and probably not significant.

*Scapula.* In both the Monroe and Anderson foci the per cent of the scapulae is close to that of the individuals represented. Rock Village is sharply set off from the other two in that the number of scapulae is less than half (45.7 per cent) of the number of individuals. Since agriculture was not practiced extensively at any of the villages the difference between Rock Village and the other two would seem to be one of butchering technique or custom.

*Forelimb.* The per cent of the proximal end of the humerus is consistently low for all sites. The Monroe focus is set off from the other two by the much greater percentage — nearly 2½ times the other two. In all three sites the proximal end of the humerus was used as a hide-burnisher and presumably many of the missing units of this element were converted into tools and worn out. Others presumably were smashed beyond recognition in the preparation of bone grease.

The Monroe focus shows the lowest per cent (45.0) of the distal end of the humerus and Rock Village the most (77.0) with the Anderson focus occupying an intermediate position (52.0). Three possible explanations offer themselves — either the people of Rock Village brought in most of the humeri for processing into bone grease, or there was less complete demolition of the bone in preparation for cooking, or the marrow was gouged out of the bone raw for the preparation of blood, berry and marrow pudding.

The Monroe and Anderson foci agree with each other and differ markedly from Rock Village in that the per cent of the proximal end of the radius is rather high. The same relationship is essentially true for the proximal end of the ulna. Possibly, because the radius and ulna carry very little meat and have a low marrow content, these elements were discarded by the people of Rock Village unless the kill was fairly close to the village.

In both the Monroe and Anderson foci the per cent of the distal end of the radius is much less than that of the proximal end. It would be much easier to cut off the metacarpal by chopping through the distal end of the radius than to "unjoint" the carpus using a stone or bone knife. That would smash the distal end of the radius beyond recognition. On the other hand the people of Rock Village had metal knives and presumably found it relatively easy to "unjoint" the carpus. Rock Village is sharply set off from the other two by the lower percentage of the scapulae, the higher percentage of humeri, and the lower percentage of radii and ulnae.

*Metacarpal.* In all three villages the per cent of metacarpals recovered is small and do not show any great differences. Since there is no usable meat and very little marrow in the metacarpal it was presumably chopped off and left at the place of butchering unless the kill was close to the village.

*Pelvis.* Rock Village is sharply set off from the other two in the very low percentage of pelves recovered, approximately one-third that of the Monroe and Anderson foci, which are nearly equal. Since the usable meat could be easily stripped from the pelvis and it contains almost no marrow it was presumably left at the place of butchering unless the kill was close to the village.

*Hind Limb.* Except for the distal end of the tibia all three villages are consistently low in the elements of the hind limb. Since the distal end of the tibia is well represented it seems reasonable to infer that the other elements were brought into the village for the marrow and presumably the missing elements were smashed beyond recognition in the preparation of bone grease.

With the exception of the proximal end of the femur the Monroe focus has the lowest per cent and Rock Village the highest with the Anderson focus occupying an intermediate position. Possibly this indicates a trend toward gouging more of the marrow out of the cavity for the preparation of blood, berry and marrow pudding or for eating raw.

The Anderson focus is sharply set off from the others by the very high percentage of astragali which is the greatest of any of the villages. (The difference in the number of the astragali and the distal ends of the tibia is small enough that it could be due to accidents of preservation or sampling.) In the Monroe focus the number of astragali is a little over half that of the distal end of the tibia and in Rock Village it is only 18 per cent. These differences seem to indicate differences in the method of severing the metatarsal from the rest of the leg. If this was accomplished by chopping through the astragalus it would be smashed beyond recognition but if it was done by chopping through the cuboid the astragalus would be undamaged.

*Metatarsal.* In all three villages the percentages of the metatarsals are consistently low and except for the Anderson focus are compatible with those of the metacarpals. This indicates that these elements were normally left at the place of butchering unless the kill was close to the village or the animals were retrieved from the river.

TABLE 12. BISON ELEMENTS FROM DODD AND ROCK VILLAGE

| Element | Number of Individuals Represented | | | Per cent of Greatest Number | | |
|---|---|---|---|---|---|---|
| | Monroe | Anderson | Rock Village | Monroe | Anderson | Rock Village |
| Skull, occiput | 6 | 2 | 26 | 10 | 5.5 | 15.8 |
| " , Horn-cores | 8 | 1 | 24 | 12 | 2.8 | 15.6 |
| " , maxilla | 12 | 15 | 62 | 20 | 41.6 | 37.0 |
| Mandible | 55 | 29 | 158 | 93.3 | 80.0 | 96.0 |
| Hyoid | 8 | 9 | 13 | 13.5 | 25.0 | 8.0 |
| Vertebra, atlas | 6 | 5 | 23 | 10.0 | 14.0 | 15.0 |
| " , axis | 1 | 1 | 18 | --- | 2.8 | 11 |
| " , cervical | 5 | 4 | 10 | 8 | 11.0 | 6 |
| " , dorsal | 3 | 2 | 11 | 5 | 5.5 | 7 |
| " , lumbar | 4 | 3 | 13 | 6 | 8.3 | 8 |
| " , sacral | --- | 3 | 13 | --- | 8.3 | 8 |
| " , caudal | 2 | 1 | 1 | 3.4 | 2.8 | --- |
| Scapula | 50 | 33 | 75 | 85.0 | 92.0 | 45.7 |
| Humerus, proximal | 12 | 3 | 14 | 20.0 | 8.3 | 8.5 |
| " , distal | 28 | 19 | 121 | 45.0 | 52.0 | 77.0 |
| Radius, proximal | 37 | 22 | 75 | 62.6 | 61.0 | 46.0 |
| " , distal | 23 | 11 | 72 | 40.0 | 30.0 | 44.0 |
| Ulna, proximal | 36 | 19 | 67 | 60.0 | 52.0 | 40 |
| Metacarpal, proximal | 17 | 11 | 37 | 28.8 | 30.0 | 22.5 |
| Pelvis | 11 | 8 | 12 | 18.0 | 23.0 | 7.0 |
| Femur, head | 10 | 9 | 36 | 17.0 | 25.0 | 22.0 |
| " , proximal | 10 | 5 | 42 | 17.0 | 14.0 | 28.6 |
| " , distal | 4 | 6 | 29 | 6.0 | 12.5 | 18.0 |
| Tibia, proximal | 5 | 5 | 48 | 8.0 | 14.0 | 30.0 |
| " , distal | 59 | 34 | 164 | 100.0 | 94.0 | 100.0 |
| Astragalus | 31 | 36 | 30 | 52.0 | 100.0 | 18.0 |
| Metatarsal, proximal | 16 | 7 | 32 | 26.0 | 20.0 | 20.0 |

## CONCLUSIONS

According to Metcalf (verbal communication) large quantities of bone splinters (presumably a by-product of the preparation of bone grease) are found in many sites in the Dakotas but such material is quite rare in Nebraska. He also suggested that if a rough estimate of the quantity of this material was recorded by the archaeologist when encountered, it would eventually be possible to delimit this trait both geographically and chronologically.

The Monroe and Anderson foci are two units of a multi-component site which yielded several rather large lots of bone which could not be assigned to any one of the three foci. Consequently the totals obtained for these two foci are probably somewhat less than would have been obtained from single component sites of the same foci. However, it is felt that if this is kept in mind while evaluating the data, useful results can be obtained.

The conclusions expressed here are based entirely on the frequency distribution of the skeletal elements without reference to the results obtained from the pottery and other artifacts.

1. The over-all pattern of the frequency distribution of the skeletal elements is essentially the same for all three villages and is interpreted to indicate culturally related peoples.

2. The frequency distribution of the skull parts from the Anderson focus is radically different from the other two, indicating very different customs in using the head of the animal.

3. There is a very close agreement between the Monroe focus and Rock Village in the frequency distribution of the skull elements indicating that, although separated by a wide time gap, they may be culturally related peoples. On the other hand, in the present state of our knowledge the possibility of the independent development of the customs expressed by the frequency distribution of the skull parts cannot be ruled out.

4. The frequency distribution of the scapulae and elements of the forelimb sets Rock Village off from the other two villages.

5. Each village expresses its own individuality in the frequency distribution of the elements of the hind limb, the forelimb and the skull.

## No. 6: THE SWEENEY ELK*

IN THE SPRING of 1951 Frank Sweeney found the tine of an antler protruding from the muck of a swamp along a little creek which traverses his farm 5 miles south of Fenton, Michigan. Digging revealed both antlers, the skull and cervical vertebrae of an elk. Further exploration with the aid of J. A. Robbins of the Genesee County Museum unearthed the pelvis, the lumbar vertebrae and a few thoracic vertebrae. No limb bones, foot bones, scapulae or ribs were found although an entire day was spent probing the muck with small metal rods. Since several small fragments of bone broken from the elements found were recovered it is believed that no major elements of the skeleton were missed.

* This paper is the joint effort of T. E. WHITE and C. M. BARBER.

The location and nature of the breaks in the bones of the skull and in the vertebrae precludes the possibility that the animal was killed and eaten by a large predator. Since the breaks in the skull are similar to those found in specimens from archaeological sites it is assumed that this elk was killed and butchered on the spot by some aboriginal hunter. The previous studies on butchering techniques have been based on the material found in villages or campsites, which represents the parts of the animal brought back after butchering. This specimen is a fortunate find in that it consists of the parts normally left at the kill.

The specimen was a rather old male. The teeth show considerable wear and the pelvic symphysis is well knit but the fusion is not complete. The ilia had not started to fuse to the sacrum. The senior author examined all of the breaks in the bones with a hand lens. Those which were the result of excavation or shipping could be distinguished from the preburial breaks very easily. (In this paper "burial" means the covering of the bone by the accumulation of sediment and by no means infers that the action of humans was involved.) In the following discussion only the preburial breaks will be considered.

*Skull.* The muzzle, consisting of the maxillae, one premaxilla, and the palatines, was separated from the rest of the skull. The line of parting follows the sutures, which indicates that the separation is the result of the decomposition possibly aided by scavengers, and not an act of the hunter. The nasals and one premaxilla were not recovered. The remaining premaxilla bears two cuts or nicks on the dorsal surface, one near the anterior end and one at about midlength. These cuts were partially filled with carbonized material which indicates that they were made at the time of skinning and butchering. The location of these cuts suggests that the hunter desired the nasal cartilages for soup, otherwise why bother to skin the head? The nasals may have been ripped from the skull in removing the nasal cartilages.

FIG. 69. Ventral view of cranium showing opening for removing the brain.

The occipital condyles and the basioccipital have been chopped away, presumably to provide access to the brain (Fig. 69). The breaks are irregular and rough, not smooth as they would have been had a steel ax been used.

*Lower Jaws.* The only parts of the lower jaws recovered are the two coronoid processes and a part of one mandibular condyle. Presumably the rest of the jaws were carried away with the tongue. After the head was partially or wholly skinned a simple procedure would be to cut the buccinator muscle on each side, force the jaws open to their widest gape and chop through the temporal muscles and coronoid process. The jaws could then be easily freed from the skull with a knife.

*Vertebra.* The vertebrae recovered are: atlas, axis, 5 cervicals, last 4 thoracic, 6 lumbar, and the sacrum. The atlas was broken in half by a blow on the top and bottom. The surfaces where the two halves join are breaks and not the smooth surfaces which would result if a steel ax or other metal tool had been used. This method of freeing the skull from the neck (by breaking the atlas in half) differs from that commonly employed on the Plains (by chopping through the joint between atlas and axis).

Only the last two of the remaining cervical vertebrae show any marks of cutting or chopping. On the last cervical both postzygapophyses and the right parapophysis have been broken off. The right pastzygopophysis of the sixth cervical has been broken off. Such rough work precludes the possibility of even the dullest steel ax having been used in butchering this elk. The neck appears to have been separated from the basket by chopping the joint between the last cervical and first thoracic vertebra.

The last 4 thoracic vertebrae were left attached to the lumbar series. Presumably, the first 9 vertebra together with the basket were carried back to camp. This differs from what seems to have been the general practice on the Plains — i.e. to chop the ribs loose from the vertebrae and leave the latter at the place of butchering. The tenth thoracic vertebra has a small abraded area on the anterior face of the centrum which suggests that the basket may have been separated from the lumbar region at this point by chopping through the joint between the ninth and tenth vertebra. The process for the tubercular head of the rib on the tenth and twelfth vertebrae is broken off on both sides indicating that the short ribs and flank were taken in for food. The diapophyses of all of the lumbar vertebra were injured before burial. The sacrum bears no mark of knife or ax. None of the caudal vertebrae were recovered. They may have been left with the hide or, being small, remained undiscovered in the muck.

*Pelvis.* The anterior external spine of the left ilium was broken off before burial. The significance of this break is not evident. The pelvis shows no other marks or breaks. The method used to separate the pelvis and femur is not evident from this material.

## BIBLIOGRAPHY


LAWRENCE, BARBARA

1951. Reports of the Awatovi Expedition. *Papers of the Peabody Museum of Archaeology and Ethnology, Harvard University*, Vol. 35, No. 3.

LEECHMAN, DOUGLAS

1951. Bone Grease. *American Antiquity*, Vol. 16, No. 4, pp. 355-6.

LEHMER, DONALD J.

n.d. Archeological Investigations in the Oahe Dam Area 1950-1951. *Bureau of American Ethnology, Bulletin* (in press).

WEISEL, GEORGE F.

1952. Animal Names, Anatomical Terms, and Some Ethnozoology of the Flathead Indians. *Journal of the Washington Academy of Science*, Vol. 42, No. 11, pp. 345-55.

WHITE, T. E.

1952. Observations on the Butchering Technique of Some Aboriginal Peoples, No. 1. *American Antiquity*, Vol. 17, No. 4, pp. 337-8. Salt Lake City.

1953. Observations on the Butchering Technique of Some Aboriginal Peoples, No. 2. *American Antiquity*, Vol. 19, No. 2, pp. 160-4. Salt Lake City.

WILSON, GILBERT L.

1924. The Horse and Dog in Hidatsa Culture. *Anthropological Papers of the American Museum of Natural History*, Vol. 15, Pt. 2.

WISSLER, CLARK

1910. Material Culture of the Blackfoot Indians. *Anthropological Papers of the American Museum of Natural History*, Vol. 5, Pt. 1.


Dinosaur National Monument
Jensen, Utah
March-July, 1953

WENDELL C. BENNETT — 1905-1953

One of the most productive careers in American archaeology was cut short when Wendell C. Bennett died of a heart attack on September 6, 1953, while swimming at Martha's Vineyard, Mass. His loss will be keenly felt not only in South American archaeology, his specialty, but also in the other branches of anthropology and in the social sciences as a whole, since his interests ranged broadly over all of these fields and he made significant contributions to all of them.

Born in Marion, Indiana on August 17, 1905, Bennett grew up in Oak Park, Illinois, where his father, William Rainey Bennett, was a Protestant minister. He attended Oak Park High School and the University of Chicago, which awarded him a Ph.B. degree in 1927, an M.A. in 1929, and his Ph.D. in 1930.

Like the other students of the period at the University of Chicago, Bennett got his start in archaeology by participating in the local survey, but this phase of his career has received little recognition, probably because he did not follow it up. Instead, he accepted an opportunity to do research in the Hawaiian Islands under the auspices of the Bishop Museum in 1928-29. Out of this came a descriptive monograph on the archaeology of Kauai and his dissertation, a comparative study of religious structures throughout Polynesia.

*(Continued on next page)*

For a year after receiving his doctorate, Bennett worked with Robert M. Zingg on a study of the Tarahumara Indians in northern Mexico, as a Research Instructor of the University of Chicago. Zingg and he did both archaeology and ethnology, but he participated only in the report on the ethnology, which was published by the University of Chicago Press in 1935. It is indicative of his versatility that he wrote the sections on social rather than material culture. From this work stemmed a life-long interest in Latin American ethnology and probably also his conviction that archaeology and ethnology are so closely related as to make it advisable, wherever practicable, to do them together.

In 1931 he went to the American Museum of Natural History to succeed Ronald L. Olson as Assistant Curator of Anthropology and to continue the program of research in Andean archaeology which Olson had inaugurated. In this capacity, Bennett made four major field trips to Bolivia and Peru, in 1932, 1933-34, 1936, and 1938, and in addition he excavated in Venezuela in 1932 as the first of three North American archaeologists invited there by Dr. Rafael Requena, secretary to the dictator of that country. All of these trips resulted in major reports.

Bennett began his work in the Central Andes at the time when Kroeber and his students at the University of California were completing their analyses of the Uhle collections and had established the first comprehensive chronology for the area. He chose as his problem the expansion and verification of their chronology. It was the logical choice, for no extensive excavation with chronology as the main objective had been done since Uhle's work a quarter of a century earlier, except for Tello's, which was not accepted because of his failure to publish the evidence.

Bennett's first two field trips were devoted to filling a gap in Bolivia, where Uhle had been unable to do chronologically significant research. In 1932, he established a sequence of three ceramic periods at the great highland site of Tiahuanaco and in 1934 he was able to distinguish four ceramic periods at Arani in the eastern lowlands and to correlate these with both the highland Tiahuanaco sequence and the results of Nordenskiöld's work in the Mojos section of the lowlands. In 1934, too, he discovered a new culture underlying Tiahuanaco-style pottery at Chiripa, southeast of the Tiahuanaco site. In 1936, he turned to the north coast of Peru and excavated mainly in Virú and Lambayeque valleys, where he substantiated and expanded the Uhle-Kroeber sequence, isolating a new Gallinazo style. Finally, in 1938 he went to the north highlands to check Tello's claim that the Chavín style is earlier in Peru than anything discovered by Uhle. He confirmed this by excavating pottery at the classic site of Chavín and placing it at the beginning of a sequence of seven ceramic periods.

Not the least of Bennett's contributions to Andean archaeology while at the American Museum of Natural History was his introduction, in the excavations at Tiahuanaco, of the technique of digging refuse deposits by arbitrary levels, in order to obtain ceramic stratigraphy, a technique which had been developed in the Southwest by one of his Museum colleagues, N. C. Nelson, and which another colleague, George C. Vaillant, was simultaneously introducing into Mexican archaeology. However, the Bolivian government was more impressed by his discovery of a large, carved stone statute in one of the strata cuts; they named this the "Bennett Monolith" and awarded him the Order of the Condor of the Andes for finding it.

While still in Peru in 1938, Bennett accepted an appointment as Associate Professor of Anthropology at the University of Wisconsin. He transferred to Yale University in 1940, becoming full Professor there in 1945 and Chairman of the Department of Anthropology in 1949. He also served as Research Associate in the Yale Peabody Museum and participated in the Museum's Caribbean Anthropological Program, which was expanded to include the area of his interests. Altogether, he made four trips to South America under Peabody Museum auspices, in 1941, 1944, 1946, and 1950, and published a major monograph on each of them. At the time of his death, he was making plans for a fifth field trip, during the spring semester of the current academic year.

For the first two trips, he decided to go to the Northern Andes in order to fill the space between the previous work of the Caribbean Program in Venezuela and the West Indies, and his own previous research in the Central Andes. He made a ceramic survey of Colombia in 1941, as his contribution to the Institute of Andean Research program sponsored by the

Coordinator of Inter-American Affairs, and in 1944 excavated in the Cuenca region of highland Ecuador, following up a survey previously made by Collier and Murra. The results of both were disappointing from a chronological standpoint — like all other field work of the period in the Northern Andes — but his reports, particularly the one on Colombia, are still the most comprehensive and readable summaries of the archaeology of the Northern Andes, and he formulated a number of hypotheses which will stimulate his colleagues in the area for years to come. In 1946, he returned to Peru to participate in the Virú Valley Program with Collier, Evans, Ford, Strong, and Willey, and his final trip was also to Peru, in 1950, when he excavated at the central highlands site of Wari in order to establish its relationships with classic and coastal Tiahuanaco.

He was one of the principal organizers of the Virú Valley Program. It is probably no accident that the locale of the Program was one in which he had previously worked and that the Program included research by ethnologists, a geographer, and a historian as well as archaeologists, in accordance with his belief in the necessity for cooperation among these disciplines. As a result, the Program conformed well to the "functional" standards which were becoming popular in American archaeology, in that the amassing of data on a relatively small area by means of a number of different approaches made it possible to draw more extensive conclusions concerning the life of the Indians and thus to add the flesh of culture to the ceramic skeleton which had been the principal result of Bennett's previous Virú research. This in turn led to the various developmental classifications which were advanced at the subsequent Conference on Peruvian Archaeology in New York and were published in the report of the Conference, "A Reappraisal of Peruvian Archaeology," which he edited.

Bennett himself never claimed much credit for these accomplishments, and his own monograph on the Virú Valley work consisted mainly of a chronological study of the Gallinazo culture, following up his previous excavations. Nevertheless, he made full use of the results of the Program in his "Andean Culture History," a *Handbook* which he wrote for the American Museum of Natural History during 1948 in collaboration with Junius Bird, and which is perhaps the best detailed synthesis of Peruvian archaeology. He also made an important theoretical contribution in his article on the Peruvian co-tradition which appears at the beginning of the "Reappraisal" volume. The article has been subject to some criticism (e.g., by the writer in another part of this issue), but it well serves the main purpose for which Bennett intended it — to express the assumptions under which Andean archaeologists are currently operating.

Bennett's courses at Yale were primarily ethnological, and he stressed the necessity for archaeologists to receive ethnological training. Limitations of space make it impossible to go into this phase of his career, except to note the volume on Argentine archaeology which he and a group of students published in 1948 as the result of a specialized seminar in the subject at Yale. Nor can his later contributions to other branches of anthropology and to outside disciplines be discussed, except in the field of area studies. He was the first Assistant Editor of *American Antiquity* for South America and the first Contributing Editor for South American archaeology to the *Handbook of Latin American Studies*. Subsequently, he served on Latin American committees at Yale and for the American Council of Learned Societies and the National Research Council, among others; and he gradually came to regard his research more as a contribution to Latin American studies than to the field of archaeology. Out of this grew an interest in area studies in general, which he promoted both at Yale and in the Social Science Research Council and which he refocused on archaeology in his 1952 presidential article for the American Anthropological Association, entitled "Area Archaeology."

Bennett was a man of strong drive and great sensitivity. He was proud of his record of prompt and thorough publication, yet he sometimes doubted the value of doing it. His feelings were well concealed behind a pleasant and unassuming manner. He was among the least jealous of anthropologists and the least inclined to impose his opinions and prejudices upon others. Few men in the profession have been so free with advice and encouragement to both colleagues and students and yet have expected so little in return. Few have left so many friends and such a record of achievement.

## BIBLIOGRAPHY

The following abbreviations are used:

| | |
|---|---|
| *AA* | *American Anthropologist*, n.s. Menasha. |
| *AAn* | *American Antiquity*. Menasha, Salt Lake City. |
| *AMNH-AP* | *American Museum of Natural History, Anthropological Papers*. New York. |
| *BAE-B* | *Bureau of American Ethnology, Bulletin*. Washington. |
| *HLAS* | *Handbook of Latin American Studies*. Cambridge. |
| *NH* | *Natural History*. New York. |
| *PAU-B* | *Pan American Union, Bulletin*. Washington. |
| *SAA-M* | *Society for American Archaeology, Memoirs*. Menasha. |
| *YUPA* | *Yale University Publications in Anthropology*. New Haven. |


1931. Archaeology of Kauai. *Bernice P. Bishop Museum, Bulletin*, No. 80. 156 pp. Honolulu.

1932. Hawaiian Heiaus and Comparative Religious Structures throughout Polynesia. A Part of a Dissertation submitted to the Graduate Faculty of the University of Chicago in Candidacy for the Degree of Doctor of Philosophy. 38 pp. Chicago: University of Chicago Libraries, Private Edition.

1932. Peruvian Gold. *NH*, Vol. 32, No. 1, pp. 22-31.

1932. Uncovering a Monolith. *Bolivia*, Vol. 4, No. 1, pp. 12-13. New York: Bolivian Consulate.

1933. Archaeological Hikes in the Andes. *NH*, Vol. 33, No. 2, pp. 163-74.

1933. Archaeological Hikes in the Andes: an Introduction to Two of the Four Great Pre-Spanish Cultures. *Bolivia*, Vol. 4, No. 7, pp. 10-15. New York: Bolivian Consulate.

1933. Review: "Fall of the Inca Empire" by P. A. Means. *The Historical Outlook*, Vol. 24, No. 4, p. 224. Philadelphia.

1934. Excavations at Tiahuanaco. *AMNH-AP*, Vol. 34, Pt. 3, pp. 357-494.

1934. Lake Titicaca. *NH*, Vol. 34, No. 8, pp. 713-24.

1934. Tiahuanaco-Inka-Chullpa: Archäeologische Ausgrabungen in den Anden. *Natur und Kultur*, Vol. 31, No. 2, pp. 47-56. München.

1935. Archaeological Horizons in Peru. In "National Research Council, Division of Anthropology and Psychology, Annual Meeting," Appendix K, pp. 1-3. Washington.

1935. Archaeological Peru. *Scientific Monthly*, Vol. 41, No. 4, pp. 305-15. Lancaster.

1935. Lake Titicaca: an Archaeologist explores the High Lake which lies between Peru and Bolivia. *Bolivia*, Vol. 5, No. 2, pp. 11-20. New York: Bolivian Consulate.

1935. Machu Picchu. *NH*, Vol. 35, No. 1, pp. 64-76.

1935. Review: Papers on Argentine archaeology by Antonio Serrano and Héctor Greslebin. *AA*, Vol. 37, No. 4, pp. 679-81.

1935. (With R. M. Zingg.) The Tarahumara: an Indian Tribe of Northern Mexico. 412 pp. Chicago: The University of Chicago Press.

1935. Weaving in the Land of the Incas. *NH*, Vol. 36, No. 1, pp. 63-72.

1936. Excavations in Bolivia. *AMNH-AP*, Vol. 35, Pt. 4, pp. 327-507.

1936 (?). Weaving in the Land of the Incas. In "Peruvian Textiles, a Traveling Loan Exhibit through the Courtesy of the American Museum of Natural History," pp. 2-13. Colorado Springs: The Taylor Museum for Southwestern Studies.

1937. The Pre-Columbian Art of Peru. In "The John Wise Collection of Ancient Peruvian Art," pp. 3-13. Hartford: Wadsworth Atheneum.

1937. Archaeological Work in South America. *AAn*, Vol. 2, No. 4, pp. 248-59.

1937. Chimu Archeology: the Archeology of the North Coast of Peru. *Scientific Monthly*, Vol. 45, No. 1, pp. 35-48. Lancaster.

1937. Excavations at La Mata, Maracay, Venezuela. *AMNH-AP*, Vol. 36, Pt. 2, pp. 67-137.

1937. The Incas and the Sun. *The Sky*, Vol. 1, No. 8, pp. 3-5. New York.

1937. Review: "Archaeology of Santa Marta, Colombia, the Tairona Culture, Part II, Section 1" by J. Alden Mason and "Archaeology of South America" by J. Eric Thompson. *AA*, Vol. 39, No. 2, pp. 342-4.

1937. Review: "La civilización Chaco-Santiagueña" by Emilio R. and Duncan L. Wagner. *AAn*, Vol. 3, No. 1, pp. 91-2.

1937. Review: "Nueva corónica y buen gobierno" by F. G. Poma de Ayala. *AA*, Vol. 39, No. 4, pp. 682-3.

1937. South America: Archaeology. *HLAS* (1936), No. 2, pp. 19-32.

1937. A Visit to the Witch of Darkness. *Transition*, No. 26, pp. 187-90. New York.

1938. If you Died in Old Peru. *NH*, Vol. 41, No. 2, pp. 119-25.

1938. Reseña de los trabajos arqueológicos en las Américas, 1934-37: I. Sud América (primera parte). *PAU-B*, Vol. 72, No. 5, pp. 288-99.

1938. Reseña de los trabajos arqueológicos en las Américas, 1934-37: I. Sud América (segunda parte). *PAU-B*, Vol. 72, No. 6, pp. 357-65.

1938. Review: "Archaeological Explorations in Peru, Part IV" by A. L. Kroeber and "Archaeological Explorations in Peru, Part III" by Lila M. O'Neale. *AA*, Vol. 40, No. 2, pp. 311-12.

1938. South America: Archaeology. *HLAS* (1937), No. 3, pp. 27-37.

1938. Summary of Archaeological Work in the Americas: I. South America [Part I]. *PAU-B*, Vol. 72, No. 3, pp. 171-80.

1938. Summary of Archaeological Work in the Americas: I. South America [Part II]. *PAU-B*, Vol. 72, No. 4, pp. 231-9.

1939. Archaeology of the North Coast of Peru: an Account of Exploration and Excavation in Viru and Lambayeque Valleys. *AMNH-AP*, Vol. 37, Pt. 1, pp. 1-154.

1939. Ceramics and Weaving in Peru. *The Popular Educator*, No. 50, pp. 4510-13. Dunellen, N.J.

1939. History of Peru. *The Popular Educator*, No. 49, pp. 4426-33. Dunellen, N.J.

1939. Peruvian Arts. *The Popular Educator*, No. 51, pp. 4611-13. Dunellen, N.J.

1939. South America. *The Popular Educator*, No. 52, pp. 4704-10. Dunellen, N.J.

1939. South America: Archaeology. *HLAS* (1938), No. 4, pp. 26-33.

1940. South America: Archaeology. *HLAS* (1939), No. 5, pp. 52-7.

1941. Review: "The Maya and Their Neighbors." *AAn*, Vol. 7, No. 2, p. 197.

1941. Review: "Tapajo Pottery" by H. C. Palmatary. *AAn*, Vol. 6, No. 3, pp. 298-9.

1941. South America: Archaeology. *HLAS* (1940), No. 6, pp. 40-6.

1942. Chavin Stone Carving. *Yale Anthropological Studies*, No. 3. 39 pp. New Haven.

1942. Conference on Bolivian Indians, sponsored by the Ethnogeographic Board, held at Yale University, New Haven, Connecticut, September 20, 1942. Washington: Ethnogeographic Board (mimeographed).

1943. The Position of Chavin in Andean Sequences. *Proceedings of the American Philosophical Society*, Vol. 86, No. 2, pp. 323-7. Philadelphia.

1943. Review: "Indians of South America" by Paul Radin. *AAn*, Vol. 9, No. 2, pp. 248-9.

1944. Archaeological Regions of Colombia: a Ceramic Survey. *YUPA*, No. 30. 120 pp.

1944. The North Highlands of Peru: Excavations in the Callejón de Huaylas and at Chavín de Huántar. *AMNH-AP*, Vol. 39, Pt. 1, pp. 1-116. New York.

1944. Review: "Historia de la cultura antigua del Perú" by Luis E. Valcárcel. *Acta Americana*, Vol. 2, No. 4, pp. 394-5. Mexico City.

1944. Review: *Revista del Museo Nacional*, Vol. 12, Nos. 1-2. *Acta Americana*, Vol. 2, No. 4, pp. 403-4. Mexico City.

1945. Interpretations of Andean Archeology. *Transactions of the New York Academy of Sciences*, series II, Vol. 7, No. 4, pp. 95-9. New York.

1945. Review: "An Introduction to the Archaeology of Cuzco" by John H. Rowe. *AAn*, Vol. 11, No. 2, p. 124.

1945. Review: "Peruvian Archeology in 1942" by A. L. Kroeber. *AAn*, Vol. 11, No. 2, p. 125.

1946. The Andean Highlands: an Introduction. In "Handbook of South American Indians." *BAE-B*, No. 143, Vol. 2, pp. 1-60.

1946. The Archeology of the Central Andes. In "Handbook of South American Indians." *BAE-B*, No. 143, Vol. 2, pp. 61-147.

1946. The Archeology of Colombia. In "Handbook of South American Indians." *BAE-B*, No. 143, Vol. 2, pp. 823-50.

1946. The Atacameño. In "Handbook of South American Indians." *BAE-B*, No. 143, Vol. 2, pp. 599-618.

1946. Excavations in the Cuenca Region, Ecuador. *YUPA*, No. 35. 84 pp.

1946. Philip Ainsworth Means, 1892-1944. *AA*, Vol. 42, No. 2, pp. 234-7.

1946. Review: "Peguche" by Elsie Clews Parsons. *Social Forces*, Vol. 24, No. 4, pp. 477-8. Chapel Hill.

1947. Conference on Peruvian Archaeology. *El Palacio*, Vol. 54, No. 9, pp. 213-16. Santa Fe.

1947. The Ethnogeographic Board. *Smithsonian Miscellaneous Collections*, Vol. 107, No. 1. 146 pp. Washington.

1948. Foreword. In "A Reappraisal of Peruvian Archaeology." *SAA-M*, No. 4, pp. v-vi.

1948. (With E. F. Bleiler and F. H. Sommer.) Northwest Argentine Archeology. *YUPA*, No. 38. 160 pp.

1948. The Peruvian Co-Tradition. In "A Reappraisal of Peruvian Archaeology." *SAA-M*, No. 4, pp. 1-7.

1948. Review: "Some Notes on the Archaeology of the Department of Puno, Peru" by Marion H. Tschopik. *AAn*, Vol. 14, No. 1, pp. 67-8.

1948. Review: "Tihuanacu, the Cradle of American Man" by Arthur Posnansky. *AAn*, Vol. 13, No. 4, pp. 336-7.

1948. A Revised Sequence for the South Titicaca Basin. In "A Reappraisal of Peruvian Archaeology." *SAA-M*, No. 4, pp. 90-2.

1949. (With Junius Bird.) Andean Culture History. *American Museum of Natural History, Handbook Series*, No. 15. 319 pp. New York.

1949. Habitations, Household Furniture, Religious Structures, Engineering. In "Handbook of South American Indians." *BAE-B*, No. 143, Vol. 5, pp. 1-65.

1949. Limitations of Archaeological Research. *Studies in Latin American Art*, pp. 22-4. Washington: The American Council of Learned Societies.

1949. Mnemonic and Recording Devices. In "Handbook of South American Indians." *BAE-B*, No. 143, Vol. 5, pp. 611-19.

1949. Numbers, Measures, Weights, and Calendars. In "Handbook of South American Indians." *BAE-B*, No. 143, Vol. 5, pp. 601-10.

1949. Research Planning: Memorandum for Rand Corporation, Crisis and Disaster Study. 17 pp. No place (mimeographed).

1949. Review: "Lost City of the Incas" by Hiram Bingham. *The American Historical Review*, Vol. 54, No. 4, pp. 904-5. New York.

1950. Cultural Unity and Disunity in the Titicaca Basin. *AAn*, Vol. 16, No. 2, pp. 89-98.

1950. The Gallinazo Group, Viru Valley, Peru. *YUPA*, No. 43. 128 pp.

1950. Review: "Archaeological Researches in the Highlands of Bolivia" by Stig Rydén. *AA*, Vol. 52, No. 2, pp. 265-7.

1950. Review: "Arqueología agustiniana" by José Pérez de Barradas. *AAn*, Vol. 15, No. 4, pp. 352-4.

1950. Review: "Indianische Kunst und Kultur" by Hermann Leicht. *Journal of American Folklore*, Vol. 63, No. 250, pp. 479-80. Lancaster.

1950. Review: "Maranga" by Jacinto Jijón y Caamaño. *AAn*, Vol. 16, No. 1, pp. 75-8.

1951. Area Studies in American Universities. 92 pp. New York: Social Science Research Council.

1951. Introduction. In "The Civilizations of Ancient America." *Selected Papers of the XXIXth International Congress of Americanists*, Vol. 1, pp. 1-16. Chicago.

1951. Review: "Races" by C. S. Coon, S. M. Garn, and Joseph B. Birdsell. *American Journal of Science*, Vol. 249, No. 7, pp. 546-7. New Haven.

1952. (With Fred Eggan, Froelich Rainey, and Sol Tax.) Anthropologists look at the World Conflict. *The University of Chicago Round Table*, No. 770. 17 pp. Chicago.

1952. Research in Cross-Cultural Education. *Items*, Vol. 6, No. 1, pp. 3-6. New York.

1953. Area Archeology. *AA*, Vol. 55, No. 1, pp. 5-17.

1953. Excavations at Wari, Ayacucho, Peru. *YUPA*, No. 49. 126 pp.

1953. New World Culture History: South America. In "Anthropology Today, an Encyclopedic Inventory," prepared under the chairmanship of A. L. Kroeber, pp. 211-25. Chicago.

1953. Review: "American Indians in the Pacific" by Thor Heyerdahl. *The New York Times Book Review* for Aug. 9, p. 1. New York.

1953. Review: "The Art of Ancient Peru" by Heinrich U. Doering. *AA*, Vol. 55, No. 2, pp. 260-1.

IRVING ROUSE
Yale University
New Haven, Conn.

# FACTS AND COMMENTS

## A NOTE ON FLUTED POINT RELATIONSHIPS

Hurt (1953, p. 211-2) quotes me as believing that the triple channel-flake pattern of the Enterline Fluted point is prototypic to the flaking from a basal nipple as noted on the Folsom Fluted, thus placing Enterline in a place typologically between Clovis Fluted and Folsom Fluted. This probably is what I should have said, and Hurt's interpretation is probably caused by my hedging on this question in correspondence, but it is not what I meant. I have made some statements on this problem elsewhere, probably with too great reservations (Witthoft, 1952, pp. 481-3, 492-4). Therefore I propose to make a more precise statement here, even though I am dealing in very conjectural matters. I have tried out these suggestions on a number of my colleagues in conversation and correspondence, and have found few of them willing to follow me far into the dimly-lighted areas of typological conjecture which follow. Nevertheless, I feel that these suggestions have some value, even if only as an argument for more extensive survey of the type-patterns involved. It will be obvious that I do not accept American lanceolate nonfluted points as local ancestors for fluted forms, but am exploring a different set of possible answers to the problem of the genesis of North American flint projectile points.

The dating of fluted-point complexes has only begun, and we have little information on their actual ages. The Lubbock, Texas date is presumed to be close to that for Lindenmeier, but it has been suggested that the Folsom Fluted point, as found at Lindenmeier and Lubbock, may cover a fairly long time range (Roberts, 1952, p. 338-9). The age of the Clovis Fluted point is much in doubt; it certainly does not belong in the same climatic stage as the Folsom Fluted point; the erosional surface between the two complexes at Blackwater Draw, and the distinctly different faunal associations, imply a considerable chronological separation. The related projectile point forms, like the Enterline type, stand without dates or geological or faunal correlations. Typology is so far the only clue to their interrelationships, and present typological studies are not far advanced. Because of this, I tried to extract all the information that could possibly be derived from the Enterline sample, in the light of the comparative material available to me. At the present time, I have handled and thought about this material so much that I have come to some sort of plateau beyond which I cannot advance without some lapse of time and some new material and ideas. Probably this is a good time to indicate the major theses to which the study of typology seemed to lead me.

The total Enterline industry, except for the fluted points, is a blade industry, and appears to me to bear closer resemblances to Old World upper paleolithic industries, and in fact to Aurignacian, than any other American industry with which I am familiar. The Clovis industry is as yet too poorly known to compare in this fashion, but the few thick blocky scrapers which I have seen do not seem to represent a similar clear-cut blade industry. These statements apply mainly to the abundant scrapers of the Enterline Chert industry, which have their parallel only in one series of the Denbigh Flint complex. "Mesolithic" traits of the Denbigh complex are missing in the Enterline complex. At the Shoop site (of the Enterline Chert industry) practically all of the flint came from an outcrop almost two hundred miles to the north; this looks like good evidence for mass movement or migration from that direction, and is a situation unparalleled at any other site that I have ever seen or heard of. I have presented the precise evidence on lithic materials and typology elsewhere, and therefore give an extension of my conclusions here. Although I express reservations about the validity of this reconstruction, it is presented in a positive fashion, because it deals with fundamentals in typology.

The Enterline Chert industry (omitting projectile points for the moment) is a blade industry without microliths, and includes pointed side-scrapers with the tip generally at the bulb-end of the blade, end scrapers with the bevel not at the bulb-end of the blade, spurred end-scrapers, bladelets, (flake knives) and uniface gravers based on flakes. Precise resemblances in any significant incidence are found only in the blade industry of the Denbigh Flint complex; there are scarcely any parallels to the microblade industry of the Denbigh Flint complex. Thus resemblances are to the part of Denbigh that is unparalleled in the described Siberian material, and not to the Neolithic of northeastern Asia. Moreover, this blade industry is very like widespread material of the Old World upper paleolithic, a technological stage so far not recognized in the New World or in northeastern Asia. In this it differs from other described paleo-Indian industries, and would appear to suggest most strongly the entry of an Old World industry into North America than any other known American industry.

When the projectile points from the Enterline industry are examined closely, many of them are found to show pronounced multiple fluting, of a type once interpreted as a decadent Folsom technique (Wormington, 1949, pp. 35, 47). A good series of specimens at all stages of completion from the Shoop site show the significance of this fluting pattern. Most, not all, of the Shoop points had first two lateral longitudinal flakes removed. These two flakes may be long and massive, or they may be very slight, or only one of them may be pronounced. They serve to shape the blank for the removal of the main channel flake; not to shape the basal contour, but the whole surface, in exactly the way that the face of a cylindrical core is shaped prior to the removal of a prism-shaped blade. Often removal of the final channel flake nearly obliterated the prior flake scars; sometimes only one of the preliminary flakes was removed, or one of them splintered out rather than spalling; however, there are so many clear-cut examples

available that there is no question as to the process involved or its significance. The man who channeled a point in this fashion was treating the blank as though it were a faceted core, and he was preparing facets when he drew the first two flakes. This is obviously an extension, an application, of blade-making, and looks like the application of the very ancient set of motor habits and tool patterns involved in blade making to the production of the fluted point, a relatively recent innovation. In terms of this, such a method of forming flutes is a very primitive one, and may prove to be a prototype for fluting in general, not an immediate prototype for Folsom fluting. Its association with very conservative flint tools of other types makes its priority over other fluted point types even more likely.

I don't know how the Clovis points were fluted, but a single aberrant unfinished specimen from the Shoop site shows the likeliest procedure. This specimen had the single channel flake drawn from a beveled, scraper-like edge (Witthoft, 1952, Pl. 2, 4). Such procedure could be intermediary between the triple channel-flake pattern of the Enterline industry and the shaped nipple striking-area of the Folsom Fluted point. Points of the Clovis type, both those of the West and the obvious examples from Pennsylvania and the adjacent areas, are longer, heavier, narrower, than the Enterline points, with straighter basal edges, more concave flutes, and generally either long and deep, or short and minimal flutes. They are not the type of the Shoop site, but are almost identical with Plains specimens. Still a third set of local types resemble the Reagan site; they seem best to fit a horizon-style that would include the Folsom and Lindenmeier site specimens. The Clovis and Shoop forms are decidedly more ancient, and have a relevance to the problems of origin of fluted-point industries that the Lindenmeier complex and the Folsom point do not have. The Shoop point could be more conservative than the Clovis point, and thus stand as a prototype, without being chronologically older, but I believe it actually is an earlier type because of some slight but important evidence in geographic distribution.

Pennsylvania and other areas of concentration of the Enterline industry are within the area of distribution of the Clovis point. In fact, a majority of the isolated finds of fluted points in Pennsylvania seem to represent the Clovis type. However, no good example of Clovis Fluted point has been found on the Shoop site. This certainly suggests a difference in age between the two forms, although it might be explained by differences between contemporary but distinct cultures. However, points which resemble the Shoop type and not the Clovis type have been found in other areas, and the present scanty data show at least as wide a geographic distribution for the triple channel-flaked points as for the Clovis type. Thus the few Alaskan specimens, discussed by Hurt and by myself, closely resemble those from Shoop, rather than the Clovis forms. In the area of Clovis point distribution on the Plains, specimens of non-Clovis form, smaller, lighter, broader, with triple channel flakes, are found (Renaud, 1931, Figs, *1, 2, 5, 9, 12 (?), 26*). Sellards considers these intermediary between Clovis and Folsom. Some of Haury's specimens from the Naco elephant site seem to represent this procedure, and none of them appear to be typical Clovis forms although it is hard to be sure from a photograph (Sellards, 1952, Pl. 22a). A single specimen from the Honduras, aberrant in many respects, still seems to show triple channel-flaking and the basal contours associated with the Enterline industry (Mayer-Oakes, 1952; Swauger and Mayer-Oakes, 1952). The most far-ranging geographic occurrences seem to represent the channeling technique which I consider earlier.

Many of us think of the nonfluted lanceolate projectile point as a predecessor to the early fluted types; such specimens are well known in many parts of the Americas, and too little is known of their age in most sections. As I understand it, however, such points are missing from actual clean-cut fluted point assemblages, as at the Clovis level of the Blackwater sites, at Lindenmeier, and at the Shoop site; where conspicuous, it is at sites with predominate later components, as at Starved Rock and the Parrish site, or at paleo-Indian sites such as Reagen and Plainview which are apparently much later than Clovis. Only at Sandia Cave is there any strong evidence for the priority of a nonfluted lanceolate point of any form, and I am personally far from convinced of the validity of the Sandia data. The correlation of the breccia with an actual Folsom point industry, the continuity of stratigraphy, the negative role of burrowing animals, and the cultural relationships of the relics are all under some shadow of doubt, and become ever more so as years pass without subsidiary evidence turning up during the rapid advance of Southwestern archaeology. If the plain lanceolate point is rejected as a presumptive ancestor to the fluted form, and considering the thesis I have stated above, a rather interesting projection backwards becomes possible.

The recognized trend is from the older triple-flaked point to the Clovis type to the Folsom type to the Plainview type to the later Solütrean-like nonfluted points with sequential chipping. If this series be an actual sequence, it still includes great gaps and covers an extremely long time span. If we venture a guess as to the next step backward before this series, we must be dealing with a very ancient complex of America or northeastern Asia which must have been in existence at least 15,000 years ago, and which is of course totally unknown. Such a complex might be very close to the beginning of flint projectile points *per se* in this region. In terms of trends in the very questionable sequence I outlined above, it might very well include projectile points which were not lanceolate points (like Solutrean points), but rather projectile points based on a blade (like the proto-Solutrean laurel leaf point). This would be parallel to the genesis of flint projectile points in several Old World sequences, and would solve the problem of the origin of a bifaced flint projectile point in blade industries of northeast Asia. Until we know

much more about pre-Folsum cultures of North America, and until we know at least a little about the pre-Mesolithic of Siberia and Mongolia, this suggestion must stand as a very unlikely guess. Shaky as my hypotheses are, they have the merit of dealing with typology as the analysis of significant processes in industries; moreover, they are based on cultural (archaeological) data rather than primarily on geological, physiographical, or paleontological evidence, and they represent an attempt to understand early stages in Amerindian cultures as parts of a world-wide picture.

As I have suggested above, I do not consider that any part of the North American continent, unless it is Alaska, can be considered as an area of prior occupation. Instead I see the continent as having been thinly populated everywhere where man could live during paleo-Indian times. Thus, I believe that the scarcely-discovered paleo-Indian complexes of the Northeast and the Southeast, like those of Alaska and of the Plains areas, include several distinct industries which pertain to a long continuum. The Northeast may have had more conservative industries than the Plains at the Clovis time level, or they both may have been occupied earlier by makers of other fluted point types. I am particularly interested in the discovery of more evidence concerning one such possibility, the Enterline Chert industry.

## BIBLIOGRAPHY


HURT, WESLEY R. JR.

1953. A Comparative Study of the Preceramic Occupations of North America. *American Antiquity*, Vol. 18, No. 2, pp. 204-22. Salt Lake City.

MAYER-OAKES, WILLIAM J.

1952. A Central American Clue to Early Man. *Carnegie Magazine*, Vol. 26, No. 6, pp. 189-91. Pittsburgh.

RENAUD, E. B.

1931. Prehistoric Flaked Points from Colorado and Neighboring Districts. *Proceedings of the Colorado Museum of Natural History*, Vol. 10, No. 2, pp. 1-22. Denver.

ROBERTS, FRANK H. H. JR.

1952. The Carbon-14 Method of Age Determination. *Annual Report of the Smithsonian Institution for the year Ending June 30, 1951*, pp. 335-50. Washington, D.C.

SELLARDS, E. H.

1952. *Early Man in America*. Texas Memorial Museum, Austin.

SWAUGER, JAMES, AND WILLIAM J. MAYER-OAKES

1952. A Fluted Point from Costa Rica. *American Antiquity*, Vol. 17, No. 3, pp. 264-5. Salt Lake City.

WITTHOFT, JOHN

1952. A Paleo-Indian Site in Eastern Pennsylvania; an Early Hunting Culture. *Proceedings of the American Philosophical Society*, Vol. 96, No. 4, pp. 464-95. Philadelphia.

WORMINGTON, H. M.

1949. *Ancient Man in North America*. Denver Museum of Natural History. Denver.



JOHN WITTHOFT
Penna. Historical and Museum Comm.
Harrisburg, Pennsylvania

March, 1952

## A COMMENT ON "FLUTED POINT RELATIONSHIPS" BY JOHN WITTHOFT

A typescript of Witthoft's article (see p. 271) was sent to me in March, 1953, with a request for criticisms. These were freely offered, and, although quite strong in some respects, accepted by him with good humor. Witthoft had himself suggested (and I agreed) that his article might be premature and should not be published until other studies of fluted points, comparable to his excellent technological study of the Shoop artifacts (see Witthoft, 1952, in Bibliography above) had been made for related complexes. He eventually decided, however, not to change his article, but to publish it with my main comments following. He believed that our disagreements would prove more stimulating to further research than no article at all.

The wording of some of Witthoft's passages is quite puzzling, and perhaps I have misunderstood his meaning. It appears to the writer that he is advancing three principal exploratory generalizations on the meaning of Shoop site fluted points and associated artifacts: (1) that the Enterline Chert industry (which includes the fluted points) is a "blade industry without microliths," and as such is unique for early man in the New World, but may have important connections with the Old World; (2) that Shoop points resemble the Clovis fluted type in some ways but constitute a new type; (3) that the Shoop points may represent the oldest fluted points in America. My brief comments will be in the same order.

1. Omitting the projectile points, Witthoft characterizes the Enterline Chert industry at Shoop as "a blade industry without microliths" which includes pointed side-scrapers, end-scrapers, bladelets (flake knives), and uniface gravers based on flakes. This use of "blade" is not that generally used in the New World, but that which is coming into use in Old World archaeology. Thus the blades are what we would call "spalls," "flakes," or (in some cases) "prismatic flakes." They are the pieces struck from cores and themselves made into uniface artifacts such as "scrapers" and "flake knives." Artifacts shaped over both faces are not "blades" but "bifaces," which would include practically all knives, axes, projectile points, etc. in the ordinary sense.

If projectile points are not omitted, the Shoop artifacts include two industries: the uniface or "blade," and the "biface." A point not made sufficiently clear is that there are biface artifacts other than projectile points at Shoop; thus Witthoft (1952, Pl. 2, Nos. 3, 5-11, 14) shows what he calls "blanks" and "knives" which would be ordinary chipped knives to most of us. Now, I think it safe to say that every artifact complex in America which includes fluted points as an established trait (this excludes the Denbigh Flint complex) also includes both these industries: "blade" and "biface." What, then, is distinctive about Shoop? Witthoft's article is a rather round-about way of emphasizing

the absence of heavy core tools such as fist axes or choppers from this assemblage. This absence is certainly striking and significant in comparison with Archaic and later cultures in the East, *but* it is also true of the fluted-point complexes in general, so far as can be said at present. Witthoft has added the phrase "without microliths" to the "blade industry" at Shoop because he sees a resemblance to Denbigh lithic artifacts in Alaska — provided one omits "microliths" from the latter. These "microliths" seem to me irrelevant to the fluted-point problem, for they are not known from any of the complexes where fluted points are established traits. Whether the Shoop site "flake knives" are distinctive is also open to question; thin side scrapers are found almost everywhere, and it is difficult to see any notable difference between the "flake knives" and "side scrapers" in Witthoft's Plate 3 (1952). If there is a distinction, this should be checked widely through these complexes to see if it is consistent.

The definition of "industries" and their tracing through time and space in all the early American remains is bound to be a critical problem. We are certainly indebted to Witthoft for broaching the subject and giving such detailed observations on the Shoop technologies. My objections, then, are against generalizing for the entire continental problem without having any other studies in any way comparable to that made at Shoop. If a chronological ordering is to be made, the details of manufacturing the fluted points are not going to be anywhere near as useful as an inventory of the associated artifacts and, where possible, geological association. Witthoft is depending far too much on a projected Shoop-Denbigh-Old World Paleolithic axial relationship without finding out enough about such points and their associations in the rest of the United States. No "Clovis industry" has ever been defined, yet we find Witthoft making comparisons between it and Enterline; and, not knowing how Clovis points were manufactured, he deduces "the method" from a single, aberrant, unfinished specimen from Shoop. In a word, let us have more studies like that for Shoop itself, and eventually we should know something about this important phase of American prehistory.

2. Whether the Shoop site fluted points are a new type distinct from Clovis depends upon how one views a type. Many archaeologists regard a type as any grouping which has even the slightest distinctions from some other grouping, and they either do not allow for variations in material and individual workmanship, or they do not know enough about related material in other areas to see the typological problem as a whole. On the other hand, one may notice a trend in archaeology these days toward defining larger types with more internal variation, *within which* there may be minor, localized specialization. Witthoft says that Clovis points are found in Pennsylvania as well as other states, but they are "longer, heavier, narrower, than the Enterline [Shoop] points, with straighter basal edges, more concave flutes, and generally either long and deep, or short and minimal flutes . . . almost identical with Plains specimens." The "Plains" specimens which have been published constitute but a small fraction of one per cent of those which even now exist in private collections. Having seen a good many of these, I am quite ready to admit that there may be other types besides Clovis and Folsom (for example, there seems to be a concentration in Tennessee of a long and very narrow form with flaring basal corners), but I do not believe that there are really significant differences between the Shoop specimens and hundreds of others from the Northeast, Southeast, Midwest, Plains, Southwest, and even California. In fact, it is truly surprising that the large and heavy fluted points which are not "Classic Folsom" are so much alike over vast areas. The eight points found with a mammoth skeleton at Naco, Arizona (reported by Haury in *American Antiquity* for July, 1953) provide an instructive example of what variety can occur in the weapons used to kill a single animal: the points vary considerably in their length, in the length of the flutes both absolutely and in proportion to the artifact length, the edges are both straight and convex, and the smaller specimens certainly do not differ much from Shoop site specimens. The multiple-fluting technique so well described by Witthoft (1952) at Shoop also appears in the Naco series, and may be expected to appear here and there whenever other Clovis points are adequately examined. Further, I think that any single one of the Shoop specimens will be matched almost exactly in other localities when such a study is made. This is merely a prediction and may be proved wrong, but I do not see how the fluted-point typology can be worked out until it is attacked on a continental basis. Nothing very valuable is going to emerge from published specimens alone, as they appear now. A state-by-state survey would eventually bring out a vast amount of information, particularly if all specimens are illustrated as Lewis and Kneberg have recently done for Tennessee (*Tennessee Archaeologist*, Vol. VII, No. 1, 1951). It is the tremendous distribution of these points that makes their analysis so difficult, for few archaeologists are able to see many of them.

3. The time has not arrived yet for anyone to make even a hypothetical case for the area of origin of fluted points. Witthoft advances some reasons for thinking the Shoop site points might be oldest, but these will not hold water. For example, the expression "The recognized trend is from the older triple-flaked point to the Clovis type to the Folsom type. . . ." cannot be true because many Clovis points are themselves "triple-flaked"; that is, the flutes were made first with two parallel longitudinal flakes removed, followed by a central flute which either destroyed the first two or left their outer edges showing. It is generally thought now that Clovis points are not only of far greater distribution than Folsom points, but also stratigraphically older in the Plains. The oldest Clovis points may be in the Plains where they occur with extinct animals, but they may eventually prove to be fully as old in the East and Far West as

well. Witthoft has also argued that the use of Onondaga chert at the Shoop site, carried from a quarry 200 miles to the north, may be possible evidence that these people were migrating southward when they picked up the chert. Did they get this chert just once or did they return to the far-off quarries from time to time? If they returned, why did they prefer this material to local flint? A similar case is seen for the people at the Folsom site in northeastern New Mexico; instead of using the plentiful obsidian in the vicinity, they obtained "Alibates flint" or dolomite from the quarries north of Amarillo, a minimum distance of 150 miles. Why? Were they migrating westward? There are other such cases, as in the older projectile points in Roaring Springs cave in southeastern Oregon, where the local obsidian was not used but other materials were obtained from far away.

Witthoft's suggestion that the Shoop site people may have been migrating from the north is closely tied to his belief in a close connection with Alaska, notably with the Denbigh Flint complex. His *assumptions* are: that the Denbigh complex is old enough to suit this requirement; that the few fluted points known from Alaska "closely resemble those from Shoop, rather than the Clovis forms"; and that fluted points are Asiatic in origin. All of these assumptions are on highly questionable ground. Collins and others have argued that, while Denbigh may be as ancient as Giddings has proposed, it may be much more recent and only slightly older than Eskimo culture; it has close ties with Dorset in stone technology, and the list of radiocarbon dates released by the University of Chicago on September 1, 1953, gives average determinations of $4658 \pm 220$ and $3509 \pm 230$ years for two samples associated with the Denbigh Flint complex (the highest of these determinations is $5063 \pm 340$ years.) Whether or not the few Alaskan fluted points are more like Shoop specimens than the Clovis type leads back to the above plea for a much broader study before such deductions are made. Whether or not fluted points are even of Old World origin is another open question. Everyone keeps repeating this assumption despite the well-known fact that none have yet been found in Asia! It is not at all improbable that they were invented in America and reached the Arctic with hunters who followed game northward with the retreat of the glaciers.[1] This possibility necessitates the hypothesis that some sort of spear or atlatl point was already present in North America, while only the fluting was a new invention. Sandia points are the only ones known as yet which might have served this role, but they are not very satisfactory in this respect. We still have a great deal to learn about the oldest American cultures.

[1] The writer discussed this problem on the program of the Society for American Archaeology at the University of Illinois, May 7-9, 1953.

ALEX D. KRIEGER
University of Texas
Austin, Texas

## PLASTER UNDER THE NEWPORT TOWER

Was the round stone tower in Newport, Rhode Island, built by the Colonists in the seventeenth century, or by the Norsemen in an earlier century? If the tower is of Colonial origin it is of interest only to local historians. If built as the beginning of a fortified Norse church before the time of Columbus, the tower is a priceless heritage of importance to all Americans.

An architectural feature of the tower which impresses the beholder is that it stands on eight columns. These mortared columns are a trifle over 3 feet in diameter. Each column rests upon a large circular stone about 3 feet 8 inches in diameter and about 8 inches thick **(1)**.* The architectural term for such a base stone is plinth. The bottom of a plinth is normally at the floor level. Since the Newport tower had no ground-level pavement, it seems fair to assume that the builders intended that the bottoms of the plinths would appear to rest upon the surface of the ground, as they appear to do today. Actually, the plinths rest upon column foundations going down to more than 4 feet below the surface **(2, 3, 4)**.

In 1948 and 1949 Godfrey made archaeological excavations beneath the tower down to the bottom of the foundations. For uncritical readers, his final report on his findings (Godfrey, 1951) would seem to "add up to a positive and unshakeable conclusion" that the Newport Tower "could not have been built before the latter half of the seventeenth century."

The entire argument turns on the date of the refill around the column foundations **(5)**. Godfrey states that as a preparation for laying the foundations a large circular construction trench was dug more than 7 feet wide, to receive the column foundations. "These foundations were then capped over with a layer of mortar, probably as a means of leveling them up before beginning the construction of the columns. It must have been necessary to fill in the construction trench before the above-ground building was begun, for the foundation stones were not mortared or fitted in any way. Apparently this refilling was done before the foundations were capped with mortar" (p. 123). Positive that the trench was refilled first, he asserts: "Excavations on the inside of several columns disclosed that the yellow-olive clay which refilled the construction trench, as well as a very thin layer of loam, actually covered part of the foundation stones before the mortar cap was poured" (p. 123). "The refilling of the construction trench took place *before* the rather extensive labor necessary to build the structure itself" (p. 126).

Having declared that the construction trench was refilled before any mortar or plaster was used to cap the foundations, Godfrey destroys his conclusions by telling us that he found bits of plaster at the bottom of the construction trench **(6)**. In fact, whenever objects of presumed or admitted Colonial origin occurred at

* Numbers in parentheses were added to permit direct reference to numbered paragraphs in Godfrey's reply which follows this paper.

the bottom of the trench or not far above the bottom, objects which Godfrey believes prove the seventeenth century origin of the tower since they "could not have reached the place where they were found except at the time of the building of the tower," it is explained that all these objects were associated with bits of plaster.

For example, (p. 127) the footprint: "Under the footprint, into the yellow clay, was a narrow slot filled with brown loam. There was no chance of mistake: the boot footprint had been covered with the yellow-olive of the construction trench. . . . Other fragments in this slot consisted of a badly rusted nail and several bits of plaster. The slot in which these objects were found was 10 inches deep at its maximum point, and about 2 inches wide. It appeared to have been made by a careless pick-stroke, and subsequently tamped in before the refilling of the construction trench. Inasmuch as the construction trench material over the trace was undisturbed and continuous to the foundation stones of the adjacent columns, the conclusion is inescapable that this deposit antedated or was contemporary with the construction of the tower."

Similarly in other places, "at the base of the southwest column" and "under the east column," where Godfrey found "a small gun flint" and a piece of Colonial glass, etc., he found fragments of plaster closely associated with them. Since he says "the foundation stones were not mortared in any way," and since he states twice that the trench had to be filled in *before* the mortar cap was poured, we see contradiction in his failure to explain how bits of plaster got down below the bottom of the trench, and into immediate association with all the finds upon which he bases his dating of the tower **(7)**.

The issues just presented were suggested by Mr. Harvey Morris of Rochester, New York, who has kindly given me permission to quote from a letter in which he points out that: "(1) A slot ten inches deep and two inches wide was quite a lusty 'careless pick-stroke' **(8)**. (2) If this stroke was made in the course of digging the construction trench, why was not the entire bottom surface covered with pick marks, instead of being 'extremely regular'? **(9)** Also (3), why just one footprint, and not many? **(10)** (4) Is it not curious that in this slot made by a single pick stroke, were found (p. 127) a fragment of the bowl of a clay pipe, a piece of a pipe stem, a rusted nail, and several bits of plaster — when not another artifact could be found nearer than the foundations, seven feet away? **(11)** Finally (5), if the construction trench was to be filled in at once, before the plaster cap could be added, why should the slot in the yellow clay be filled in — not with yellow clay, but with brown loam? And then be tamped down with a boot?" **(11, 12, 13)**.

It is difficult to accept the idea of a "construction trench" that had been dug "more than seven feet wide" **(14)** in a circle over twenty-four feet in diameter, to receive the column foundations, when those column foundations, like the columns above them, are only a trifle over three feet in diameter **(15)**. Instead, such a trench suggests rather strongly someone's systematic search for anything and everything of interest that might lie buried anywhere near the tower **(16)**.

Godfrey relies on the outside edge of the construction trench in arguing that because he was careful in his digging, he cannot be mistaken in his conclusions. He says (p. 127): "The outside edge of the construction trench was known and already photographed, so there was no possibility, with the care with which we were excavating, that it would be missed." Godfrey evidently cannot rely on the inside of the construction trench, of which he says: "The inside edge may have been scalloped inward to receive the footings or may have been irregular." The "may have been" implies uncertainty **(17)**. In other words, we cannot be sure of it. Its irregularity *may have been* the irregular edge of the repeated diggings by treasure hunters and of their refills. Why would the edge of a trench over seven feet wide need to be "scalloped inward" to receive footings which were considerably less than four feet in diameter? **(18)**

On page 120 of his report, Godfrey makes an amazing statement: "It was to the advantage of newcomers to the area to claim a greater antiquity for the structure than the descendants of the early settlers would allow **(19)**. Even today it is possible to predict, with considerable accuracy, which opinion any local inhabitant holds of the tower's origin, if one knows the religion, ancestry and wealth of the individual." Here is something new in archaeology: Godfrey's theory that a man's opinion of the age of a structure is predictable upon knowledge of that man's religion, ancestry and wealth **(20)**. Godfrey does not tell us which religions tend to cause inhabitants of Rhode Island to believe in the Colonial theory of the tower's origin and which religions make one susceptible to a belief in a Norse origin. Presumably, Scandinavian ancestry does predispose a man to belief in the Norse theory, but there are many non-Scandinavian inhabitants of Rhode Island who also believe in that theory. It would be interesting to know how wealth or lack of wealth affects the beliefs of the local inhabitants in regard to the date of the origin of the tower.

Godfrey does not believe in the widely-accepted mill theory of origin of the tower **(21)**. He admits as I do, and as everyone who looks into the records must admit, that the tower was once *used* as a mill. But Godfrey believes it was *built* for a different purpose. He says it was built by Governor Arnold as "a summer house or 'folly.'" He says that Holand's "altar slot" inside the tower was at the height of a writing desk of the kind in use until the end of the nineteenth century. This interesting suggestion would seem to be as valid as the "altar" theory, until we realize that the summerhouse or "folly" theory brings us into conflict with the facts of the fireplace flues in the tower wall. Those flues go up from the fireplace for about twelve feet and turn at right angles to openings in the wall. These flues are in a form used in the thirteenth and fourteenth centuries, a form discarded upon the invention of chimney flues

extending through the roof of a building to make an effective draft.

For those who repudiate the theory of fourteenth century Norse origin of the tower, the only explanation for the position of the flue openings is that they had to be where they are if the tower was built to be a windmill. In such case, chimney openings would be impossible at the top of the tower because the vane and the mechanism supporting the sails of a windmill would there have to turn on a collar. In denying the windmill theory of origin of the tower, Godfrey casts aside the only explanation yet given for the position of the flue openings other than the explanation that the flues were in fact built in the fourteenth century when such a form was in vogue. What explanation does he offer after repudiating the two rational ones? Does he ask both schools of thought, the Colonial and the Norse, to accept the all-permitting "folly" instead of something rational? **(22)**.

Analysis of Godfrey's report on his excavations beneath the Newport Tower raises grave doubt that he has proved anything in regard to the age of the structure **(23)**. All he seems to have proved is that the whole area has been worked over, again and again. As he says (p. 126), the tower has for centuries been a focal point for treasure hunters, and the area is pitted with innumerable holes **(24)**. The only reasonable explanation for his finding of fragments of Colonial objects always in association with pieces of plaster is that those objects found their way to the spots where he found them during the churning of repeated diggings and fillings by treasure hunters and by Governor Gibbs' excavators.

## BIBLIOGRAPHY

GODFREY, WILLIAM S.
1951. The Archaeology of the Old Stone Mill in Newport, Rhode Island." *American Antiquity*, Vol. 17, No. 2, p. 120-9. Salt Lake City.

FREDERICK J. POHL
141 Columbia Heights
Brooklyn 1, New York
April, 1953

## ANSWER TO "PLASTER UNDER THE TOWER"

The criticisms from Pohl imply that he has refuted certain contentions which I have made concerning the Newport Tower. I have refrained from anything beyond comment on his various statements.

1. Only three of the columns (N, NE, E) rest on circular stones. The stone on which the SE column rests is only partially circular, the missing portion being chinked out with smaller stones. The other four columns rest on "plinths" (see point 3 below) which are composed of many small stones (Means, 1942, and personal observation).

2. The stones, and the column bases, are very close to 4′ in diameter, and probably were 4′ in diameter when the mortar coating which originally covered them was in place.

3. Pohl's discussion of plinths is completely immaterial. The columns did not stand on plinths, but on drums 18″ high, which are now buried in the soil with the exception of their top stones. We were able to identify the original floor level in 1949, and to determine that the original floor surface was gravel (Godfrey, 1950b, 1951). The drums were mortared and plastered (or whitewashed) down to this floor.

4. The column foundations under the drums go down to varying depths according to the hardness of the subsurface, but never as much as 4′.

5. My "argument" also includes one fragment of clay pipe found under a column (see Pohl, 1950 and my reply, 1951a).

6. At no time whatever did I suggest, state, or presume that the mortar (or plaster) was mixed only after the refilling of the construction trench. In fact, there is ample evidence that the mortar *was* mixed before the trench was filled. Mortar fragments, showing no surfaces, and hence not fallen from the disintegrating tower (as were most of the mortar fragments found in pot hunters' excavations), were found all through the fill of the construction trench, and provided the means of identification of the construction trench fill in 1948 (Godfrey, 1948, 1949).

7. If mortar fragments were present throughout the construction trench fill, it is not surprising that they were in association with the artifacts. In the fill also, and also in association with datable artifacts, were many rusted iron nails, which were too fragmentary to be dated.

In reply to Mr. Harvey Morris of Rochester, New York, may I say:

8. (1) A 10″ pick stroke is hardly "lusty" into material as soft as yellow clay. The position of the stroke, near the edge of the trench where the trench bottom was curving upward into the trench side, is just the place where careless error could most likely be made.

9. (2) Perhaps much of the bottom was covered with pick marks — but it is not likely. Pohl (or Morris) should dig a trench some time, using a pick and shovel. They will find that pick marks are not a feature of the trench bottom. Archaeologically, the inch-by-inch examination of a trench bottom is an exceedingly tedious and unrewarding task. However, this was done in two places, and in both as was reported, we found tool marks (Godfrey, 1950b).

10. (3) Perhaps there were other footprints. We found only one, and that only because of very careful excavation on a very restricted area. Nature of the soil, time for excavation, and available funds did not permit such detailed digging of the rest of the area.

11. (4) This is a rhetorical question which admits no positive answer. In a specified area of the excava-

tion, certain finds were made. Nails and mortar were found in the fill in the vicinity, but, since such items were not datable, they were not reported in detail. Over 20,000 artifacts were found in the excavation (Godfrey, 1950, 1951b). To enumerate in a report the position of each, or even to record each, would have been absurd.

12. (5) There is no contention or indication that the construction trench was filled in immediately after its excavation. Whether it was or not, there is no reason apparent for the selection of brown loam (except, perhaps, its availability) for the refill of the pick hole. The evidence appeared in the excavation; I reported it.

13. Dating the tower does not rest on this footprint alone. Even if the evidence of the footprint be disregarded, there is other evidence of Colonial origin, *and not one scrap of evidence for any other origin of the structure.*

14. Why is a 7' construction trench "difficult to accept"? The foundation stones of the columns filled the trench from the outer to the inner edge (some of them were even pushed into the inner edge, we suspect) and only left unfilled the parts of the trench between the columns. Pohl might have used a narrower trench; the tower builder, however, elected to use this size.

15. The tower foundations are definitely not "a trifle over three feet in diameter." They fill the construction trench. This point has been emphasized in all my published articles.

16. There is absolutely no record of "systematic search" around the tower. Pohl states that the evidence of the construction trench which we found represents "someone's systematic search . . ." in other words, he suggests that the whole construction trench, "more than seven feet wide in a circle over twenty-four feet in diameter," consolidating the unmortared foundations of a tower weighing many tons, was removed in the 18-20th century, *and replaced as a unit*, or replaced in such a way that the demarcations between the fills of the various units by which it was removed could not be observed. Nonsense. If Pohl knew anything of the history and techniques of archaeology, he would realize that the excavation of such a trench, *today* would be a practical impossibility, and that such an excavation could not have been dreamed of before 1855, when Touro Park was established, and the tower fenced off.

17. Inspection of the tower, knowledge of archaeological techniques, and perusal of the restrictions established by the Newport Park Commission (Godfrey, 1950) would make it quickly apparent to anyone with experience why it was difficult to obtain an adequate cross-section of the inside of the construction trench. Also, the inside edge had been much damaged by pot-hunting (Godfrey, 1950).

18. If it were scalloped, the reason is yet unknown. Evidence *suggested* that the inside edge was scalloped. Evidence was positive that each column rested in a depression at the bottom of the construction trench. Why dig a construction trench with a smooth but undulating bottom, and then sink eight small depressions in the bottom to receive the column foundations? The evidence for the latter action is clear; the reasons are unknown.

19. As an added example, I cite Booth Tarkington's *Mary's Neck* for a humorous discussion of the relationship between "natives" and "summer visitors." The statement may be "amazing," but it is the result of repeated observation.

20. Pohl's innocent merriment anent the relationship between social position and ideological concepts is hard to swallow. For comparable differences of opinion (although on a different subject!) based on religion, wealth and education, he might read Kinsey, Martin and Pomeroy: *The Sexual Behavior of the Human Male.* The complex factors involved in the Newport situation are still under study. The study has no bearing on the *archaeology* of the Newport Tower, but it certainly concerns the *anthropology* of Newport.

21. When my article was written, I was doubtful about the mill theory. Today, I am not so sure. Rex Wailes, noted English expert on windmills, has re-examined the evidence for the Newport structure and the suggested prototype in England, Chesterton Mill (Wailes, in press), and has supported the contention that both structures were built as mills.

22. Pohl's argument concerning the flue-openings is specious when one realizes that there are many "archaic" features in the whole tower, only one of which is the flues. We are still guilty of such archaeisms when we build Gothic Cathedrals (St. John the Divine, N.Y.C.), museums in the form of Greek Temples (Philadelphia Art Museum), and railroad stations modeled on Roman Baths (Pennsylvania Station, N.Y.C.). No one contends that these structures were built by the French, Greeks or Romans!

23. The excavations produced certain evidence: a) Positive: the association of Colonial artifacts with the foundation, in one instance directly under a column; and b) Negative: no trace of anything earlier than Colonial (Viking, Irish, Welsh or other), and c) no stratigraphical break indicating that such evidence had been removed.

24. Pohl's "only reasonable explanation" is not in accordance with the facts. The presence of mortar fragments in the construction trench fill has already been announced twice (Godfrey, 1948, 1949). The problem of the detection of pot-hunters' pits has likewise been analyzed (Godfrey, 1950, 1951b).

Pohl's objections to the excavation around the Newport Tower seem to stem from two sources: lack of adequate understanding of archaeological methods and techniques, and a basic refusal to accept any evidence which conflicts with his preconceived notion of the tower's origin. No one today can say with absolute

finality that the tower was built in a certain year and by a certain person, but the weight of concrete evidence favors heavily the presumption of Colonial origin.

## BIBLIOGRAPHY

GODFREY, WILLIAM S., JR.

1948. *Report to the Annual Meeting of the Preservation Society of Newport County.* Issued in mimeograph, distributed by the Newport Chamber of Commerce, 1949.

1949. The Newport Puzzle. *Archaeology*, Vol. 2, No. 3, pp. 146-9. Cambridge.

1950. Newport Tower II. *Archaeology*, Vol. 3, No. 2, pp. 82-6. Cambridge.

1951a. The Newport Tower, A Reply to Mr. Pohl. *Archaeology*, Vol. 4, No. 1, pp. 54-5. Cambridge.

1951b. The Archaeology of the Old Stone Mill in Newport, Rhode Island. *American Antiquity*, Vol. 17, No. 2, pp. 120-9. Salt Lake City.

MEANS, PHILIP AINSWORTH

1942. *Newport Tower.* Henry Holt, N.Y.

POHL, FREDERICK J.

1950. The Newport Tower: An Answer to Mr. Godfrey. *Archaeology*, Vol. 3, No. 3, p. 183. Cambridge.

WILLIAM S. GODFREY
Beloit College
Beloit, Wisconsin
August, 1953

## CARVED ATLATL FROM NORTHWEST COAST

On June 7, 1952, the writers were shown the artifact, seen in Figure 70, by Mr. and Mrs. Edwin Johnson of La Conner, Washington. According to the informants, the artifact was dredged from the mouth of the Northwest distributary of the Skagit River near its confluence with the Straits of Georgia (about forty-five miles north of Seattle).

The artifact is about forty centimeters long and approximately eleven centimeters in maximal height. It is carved from wood of an unknown origin. The raised grain and deep checks suggest long continued immersion subsequent to the execution of the carving. The two holes and extended groove along the flat surface identify it as an atlatl, at least in form. However, the mass of raised carving suggests that the artifact may have served a functionally different purpose. It should be noted that the carving would serve as a stop. The photograph serves in some measure to describe the carvings.

None of the authorities we have consulted thought the carvings to be at all typical of the Northwest Coast. It should also be noted, parenthetically, that the atlatl, *per se*, is unknown from the Northwest Coast. A cursory examination of the literature revealed:

1. No similar carvings upon the atlatls or spear-throwers of any area.

2. No clear-cut similarity in design and/or design elements.

3. The two-finger grip is characteristic of some Mesoamerican atlatls (hulches), and is possibly found in the Arctic area.

There would seem to be four ready explanations for the presence of this cultural incongruity in the mud of the Skagit bottom:

1. It drifted in and was caught in tidal or river eddies, became waterlogged, and sank.

2. The artifact was used locally and deposited before the extant meander pattern of the North Skagit, or subsequently was lost.

3. The artifact is a cultural intrusive of uncertain derivation.

4. The artifact is a modern fraud.

The condition of the artifact, which is evidently unique would seem to militate strongly against the acceptance of the fourth alternative. The turbulent water conditions prevalent in the Sound area make it unlikely that the atlatl could have drifted far in such a relatively undamaged condition. The acceptance of alternative 2. or 3. would have important implications in the archaeological reconstruction of the Northwestern area.

FIG. 70

If permission may be obtained from the owner, it is proposed to undertake a series of analyses directed toward a more objective presentation of the problem.

HERBERT C. TAYLOR, JR.
Western Washington College of Education
Bellingham, Washington

WARREN CALDWELL
University of Washington
Seattle, Washington
March, 1953

## NOTES ON THE THIRD INTERGLACIAL ARTIFACTS

The following is a discussion of Carter's (1952) third interglacial artifacts. Carter's conclusion that he has found third interglacial artifacts is based on two hypotheses: 1. The objects found are artifacts; 2. The layer in which they were found dates from the third interglacial. These comments are limited to a brief discussion of the first hypothesis.

The pictures and descriptions furnish no concrete proof that the finds are artifacts, although the burden of proof must rest with him. We contend that these "artifacts" are well within the range of naturally chipped stone. This conclusion is based on Barnes' (1939) discussion of natural and artificial flaking of flints which is based on his own work and that of the Abbé Breuil, S. H. Warren and others. Barnes and Breuil have shown that natural forces can produce flaked flints, many of which are far more convincing "artifacts" than those Carter reports.

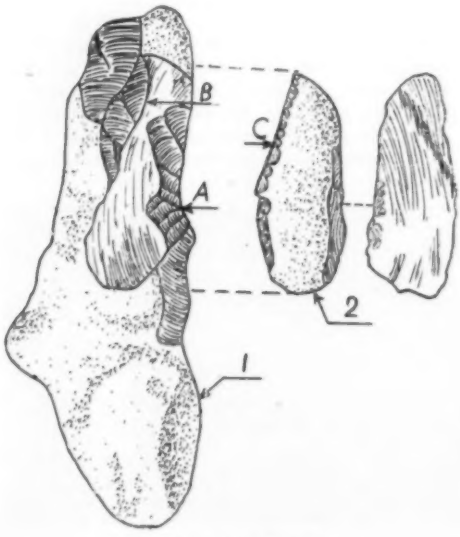



FIG. 71. (After Breuil).

According to Barnes (1939, p. 103)

> There is no doubt that in certain localities, such as sea beaches, fortuitous concussion can flake flints freely. It must be admitted that the bombardment of stones fixed in the face of a cliff or in the bed of a river by other stones . . . will sometimes produce parallel flaking.

We therefore question Carter's statement (1952, p. 452) "that beaches and stream beds do not produce flaked stone." Beach chipping occurs on the fixed stones and can produce sharp edges on them. The net result on the rolling stones will, of course, be to blunt or dull any sharp edges if the rolling is carried on for sufficient time.

Barnes and Breuil show flints, from Eocene deposits, which were flaked by "pressure produced by the foundering of a deposit due to the dissolution of an underlying bed" (Barnes, 1939, p. 103). Many of these flints show even parallel flaking. Breuil (1910, p. 403) explains the formation of the flake and core (Fig. 71) as follows: The pressure of the soil (brought to bear on (A) by impinging on another stone) has detached a flake (2) from the core (1). The flake is pushed away from the core at (A) and is ground against the core at (B) with a rotary motion. This action removed a row of chips from edge (C) of the flake. Both the flake and core were found together in an Eocene level.

Figure 72 shows a "scraper," from an Eocene layer near Paris, which was produced by the natural forces just described. Carter's "retouched flake" (1952, Pl. 4, *c,d*) is a much cruder and less convincing "artifact" than these Eocene flints.

Breuil (1934) has shown how solifluction may produce flaked flints. Barnes (1939, Fig. 3) illustrates a core so chipped, from which 27 parallel flakes had been removed! The base of this core is flat and shows a good bulb of percussion. This naturally flaked flint (several of the detached flakes were still in place) far surpasses in complexity Carter's most convincing "artifact" (1952, Pl. 4, *g,h*).

Thus it appears that, contrary to Carter's opinion, nature can produce sharp edges and uniform, localized, parallel flaking. This places his "artifacts" well within the range of naturally chipped stone. He describes the "artifacts" as flakes and cores which, as we have shown, can be produced by natural forces. In all fairness, we must admit the existence of a category of artifacts which we will designate as casual artifacts. This would

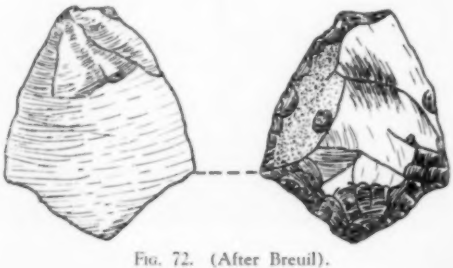


FIG. 72. (After Breuil).

include unaltered flakes used as tools and unused flakes and cores which — if man-made — are technically artifacts. The question arises as to whether such artifacts could be identified as man-made, especially after some rolling in a stream bed.

Such casual artifacts can be identified only by their association with man. Carter's "artifacts" may fall in this category, but they do not occur in association with man or any recognizable man-made objects. Hence, their status as artifacts is doubtful. Therefore, it seems highly probable that the "artifacts" found by Dr. Carter are not man-made, but naturally chipped flints.

## BIBLIOGRAPHY


Barnes, Alfred S.

1939. The Differences between Natural and Human Flaking on Prehistoric Flint Implements. *American Anthropologist*, Vol. 41, No. 1, pp. 99-112.

Breuil, Henri

1910. Sur la présence d'éolithes à la base de l'Éocéne Parisien. *L'Anthropologie*, Vol. 21, pp. 395-408.

1934. De l'importance de la solifluxion dans l'étude des terrains quarternaires du Nord de la France et des pays voisins. *Revue de Géographie Physique et de Géologie Dynamique*, Vol. 7, Pt. 4, pp. 269-391 — as cited in Barnes (1939).

Carter, George F.

1952. Interglacial Artifacts from the San Diego Area. *Southwestern Journal of Anthropology*, Vol. 8, No. 4, pp. 444-56.



William B. Roosa
Stewart L. Peckham
Department of Anthropology
University of New Mexico
Albuquerque, New Mexico

April, 1953

## ARTISTIC CONNECTIONS BETWEEN THE CHICHEN ITZA TOLTEC AND THE CLASSIC MAYA

A recent development of some interest in Maya archaeology has been the appearance of several studies showing rather unexpected connections between the art of the Toltec period at Chichen Itza and that of the late Great period in the Classic centers to the south. Inferentially, this may indicate the approximate contemporaneity of the Chichen Itza Toltec and the late Classic Maya. Such a reconstruction is not only a far cry from the outmoded concept of "Old" and "New Empires" but, as Lothrop (1952, p. 112) points out, challenges the generally favored 11.16 (Goodman-Martinez-Thompson) correlation of the Maya and Christian calendars. Three studies may be cited as of particular importance in emphasizing the affinity of Chichen Toltec and Classic Maya art. Monographs by Proskouriakoff (1950) and Lothrop (1952) deal, respectively, with sculptured monuments from the Maya area as a whole and with metal objects from the sacred cenote at Chichen Itza. A paper by the present writer (Rands, 1953) is concerned with portrayals of a single motif, the water lily, in the Maya area.

Proskouriakoff makes several contributions to the present discussion. For example, she demonstrates the existence of a bewildering variety of "basically Classic" and "non-Classic" artistic traditions in Yucatan (Proskouriakoff, 1950, p. 156; 1951). While the precise implications of her data are not at all clear, it is apparent that the complexities of the historical situation were great, making suspect any highly simplified reconstruction. More specifically, she isolates motifs transitional to those of the Toltec, suggesting that Toltec immigrants may have been in Yucatan "for some time before they established themselves as a dominant group at Chichen Itza or that they were settled for a time in some peripheral location where they were in contact with the people of Yucatan" (Proskouriakoff, 1950, p. 170). Perhaps the Toltec were preceded in Yucatan by other non-Maya peoples (Proskouriakoff, 1950, p. 183). Finally, the existence of artistic elements suggesting the Toltec period of Chichen Itza are documented on late monuments at several Classic sites; foreign influences seem especially notable at Seibal (Proskouriakoff, 1950, pp. 152-153).

Lothrop's findings reveal that the central panels on numerous gold disks from the sacred cenote at Chichen Itza were executed in the art style and subject matter of the Toltec, the remainder of each disk being "completely" in the artistic tradition of the Maya Great period. He suggests that such repeated associations, plus various Yucatecan carvings, indicate that the Maya Great period, the Puuc period in Yucatan and the Toltec of Chichen Itza "were in part contemporaneous and flourished together for a considerable period of time" (Lothrop, 1952, pp. 111-112).

In analyzing Maya floral motifs, I have emphasized resemblances on an intersite or regional basis but have not gone on to infer temporal correspondences from these data. Conceivably, this could be done in the case of representations of probable water lilies at Palenque and in the Toltec art of Chichen Itza. In any event, the portrayals of this motif at the two centers are exceptionally similar, while other sites in the western part of the Maya area display additional resemblances of note to the Mexican period examples at Chichen Itza (Rands, 1953, pp. 120-121). Interestingly, but of unclear significance to the problem, Proskouriakoff and Thompson (1947) suggest artistic and calendric features linking the Puuc region in northern Yucatan with certain Classic sites in the Usumacinta Basin. Seemingly, the data concerning the water lily motif could suggest an initial locus of Toltec contacts with the Great period Maya in the western periphery of the Classic area, a region lying in the direction of Tula and hence, perhaps, especially subject to contact with peoples stemming from highland Mexico.

It is recognized that sweeping historical reconstructions cannot be made simply on the basis of representational art, let alone on so specialized a trait as a floral motif. Nevertheless, the three artistic studies which have been cited are of considerable interest. Although dealing with quite distinct subject matter, they do point insist-

ently to a stronger connection between the Toltec of Chichen Itza and at least some of the Classic Maya than has generally been supposed.

## BIBLIOGRAPHY


Lothrop, Samuel K.

1952. Metals from the Cenote of Sacrifice, Chichen Itza, Yucatan. *Memoirs of the Peabody Museum of Archaeology and Ethnology*, Vol. 10, No. 2. Cambridge.

Proskouriakoff, Tatiana

1950. A Study of Classic Maya Sculpture. *Carnegie Institution of Washington*, Publication 593. Washington.

1951. Some Non-Classic Traits in the Sculpture of Yucatan. *The Civilizations of Ancient America. Selected Papers of the XXIXth International Congress of Americanists*, pp. 108-18. Chicago.

Proskouriakoff, Tatiana, and J. Eric S. Thompson

1947. Maya Calendar Round Dates such as 9 Ahau 17 Mol. *Notes on Middle American Archaeology and Ethnology, Division of Historical Research, Carnegie Institution of Washington*, No. 79. Cambridge.

Rands, Robert L.

1953. The Water Lily in Maya Art: A Complex of Alleged Asiatic Origin. *Bureau of American Ethnology, Bulletin 151 Anthropological Papers*, No. 34. Washington.



Robert L. Rands
Department of Sociology and Anthropology
University of Mississippi
University, Mississippi

April, 1953

## ADDITIONAL NOTES ON THE POVERTY POINT SITE IN NORTHERN LOUISIANA

The two large mounds at the Poverty Point and Motley sites in West Carrol Parish, Louisiana, were first adequately described by C. B. Moore (1913, Fig. 29), and for a long while these structures and the cultural remains scattered about them remained one of the principal puzzles in the archaeology of the lower Mississippi Valley. Clarence Webb has made extensive surface collections from this locality for a number of years and his three articles in *American Antiquity*, the last written with Haag, form the bulk of the information which we have on the culture (Webb, 1944, 1948; Haag and Webb, 1953). The purpose of the present brief note is to report some newly discovered facets of the Poverty Point cultural complex.

The writer was able to work a few weeks at the site in the spring of 1952 and again in 1953. However, the most remarkable discovery was not made in the field but in the Cartographic Laboratory of the Mississippi River Commission in Vicksburg. An examination of air photographs showed clearly that in the fields where most of the cultural refuse lies, to the east of the large Poverty Point mound, there is an arrangement of six concentric earth ridges forming a portion of what probably was originally a complete octagonal figure three-quarters of a mile in diameter. This figure has not previously been discovered by observation on the ground simply because it is on too large a scale.

A map of the Poverty Point site, made as a tracing from an enlarged air photograph is given in Figure 73. As can be seen, the large peculiarly-shaped Poverty Point mound is located one-half mile west of the bluffs over Bayou Macon. The shape of this structure is shown in detail by Figure 74, a plane table map which was made in 1952. This is a somewhat more accurate representation than the sketch map published by Moore but basic measurements are in agreement. The highest part of the mound is between 65 and 70 feet above the surrounding almost flat terrain. In the spring of 1953, the writer sank a 4 inch bore hole 60 feet deep into the mound from its highest point. Varicolored soil, indicative of basket loading, was encountered for the entire depth of the hole. This seems to eliminate the possibility of a pre-Wisconsin erosional remnant having been utilized as a core for this structure.

In the site map, Figure 73, the draftsman has shaded the artificial ridges which form the portion of the octagonal figure to conform to the air photograph—the tops of the ridges are indicated by light areas and the swales lying between ridges are darkened. The ridges are rounded elevations from 4 to 6 feet high and the tops of these elevations are approximately 100 feet apart. At the corners of this fragment of an octagonal figure are gaps in the ridges, gaps which form a sort of aisle leading toward the center of the figure. Most of the area of these six concentric ridges is in cultivation and in these fields is found a profusion of the baked-clay "Poverty Point objects," flint scraps, and other occupation refuse that has been described by Moore, Webb, and Haag. In 1952 the writer ran 5-foot wide trenches through the ridges at three points. They are obviously artificial and the fill contains quantities of dwelling refuse, with an occasional fireplace, but no alignments of postholes or other positive evidence of buildings were found. However, the excavations were not extensive enough to be certain that building remains will not be found; it appears obvious that the ridges were extensively utilized as dwelling sites.

A notable feature of the Poverty Point site and the related Motley mound is the orientation. As is shown by Figures 73, 74, the Poverty Point mound lies almost exactly to the west of the concentric octagonal ridges. The high narrow ridge on the mound is aligned north-south. In addition the Motley mound, not shown on these maps, is situated one and one-half miles almost exactly north of the center of the fragmentary octagonal figure. Like the Poverty Point mound, it has a high ridge, the crest of which slopes each way from a central eminence that is 51 feet high. The ridge of this mound runs east and west and on the southern side, toward the octagon, there is a wide sloping apron suggestive of the platform of the Poverty Point mound but not flat on top. Borrow pits made in the side of this structure by local farmers show basket loading of soils.

The relationship of the Poverty Point site to the physiographic history of the Mississippi alluvial valley as developed by Fisk (1944) has been worked out in some detail but is too involved to report in this brief

note. Briefly, both the large mounds and the ridges forming the fragment of an octagon are on Macon Ridge, a level surface that is elevated 15 to 20 feet above the Mississippi floodplain to the eastward. Macon Ridge is an outwash fan formed by the Arkansas River in Fisk's stages $A_1$ to $A_3$. The site happens to be on an $A_1$ portion of the surface. The estimated age for this surface is about 4000 B.C., thus giving a limit to the age of the constructions.

In Fisk's stage $C_1$ the Mississippi River, at that time a braided stream, moved westward and trimmed the edges of the Arkansas outwash fan until the bluffs separating this higher surface from the Mississippi floodplain were only from one-half to three-quarters of a mile to the east of the center of the octagon. Stage $C_1$ (estimated age about 1500 B.C.) is a possible dating for the constructions and it is particularly attractive in view of the excellent evidence for relating this same cultural complex to a $C_1$ course of the Ohio River at the Jaketown site in Mississippi.

The upper limits for relating Poverty Point with the river channel chronology is provided by a Stage H course of the Arkansas River which further trimmed the edge of Macon Ridge cutting away a portion of the octagonal earthworks. Stage H dates approximately 500 B.C. Further cutting into the bluffs upon which the northern segment of the octagon is located was done by a meander of Bayou Macon. This meandering appears to have taken place in Stage I (100 A.D.) when this bayou was an active diversionary channel for the Arkansas River.

The question of the actual age of the Poverty Point complex is far from settled. While Fisk's channel chronology is plainly a valid sequence, the exact age estimates depend on a number of factors, and, as Fisk is the first to insist, will have to be modified as evidence accumulates. The one radiocarbon date which we have for the Poverty Point complex at the Jaketown site is $2350 \pm 80$ years (Kulp et al., 1951, sample 114). If this complex does date about 400 B.C., then some considera-

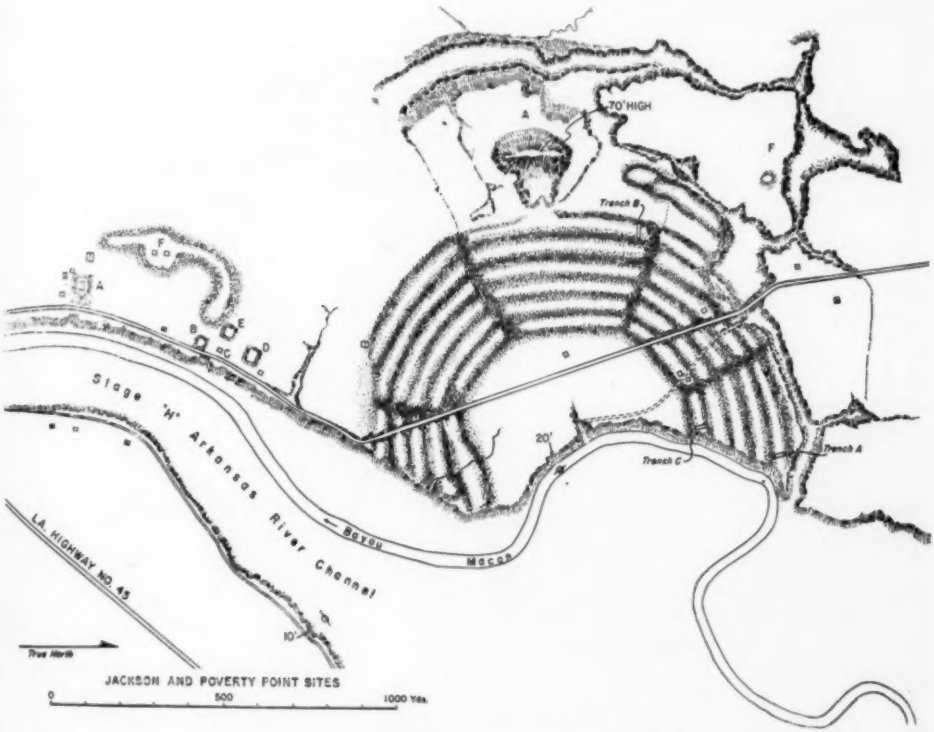



FIG. 73. Map of the Jackson and Poverty Point sites traced from an enlarged air photograph. The Jackson site dates in the Troyville and Coles Creek periods. The low ridges to the east of the large Poverty Point mound A are indicated by the same shading that shows in the photograph; the swales between ridges are dark while tops of the ridges are light. The locations of three trenches dug across these ridges are shown. The small rectangles are modern farm houses.

tion will have to be given to changing the estimates for the river channel chronology.

It has been a matter of considerable surprise to the writer, at least, that such large and complex earthworks should have been built by preceramic peoples at such an early date in the lower Mississippi Valley. Additional research will doubtlessly clarify the problem, but it is not too soon to offer a few surmises. It has occurred to the writer that perhaps the two peculiarly shaped mounds are large bird effigies, the high peak representing the head, the flanking ridges the wings, and the platform the tail, while the resemblance of the octagonal figure to the earthworks at the Newark, Ohio, Hopewell group is obvious.

A possibility which deserves consideration is that the date obtained at Jaketown and the Ohio and Illinois Hopewell sites dates may be substantially correct. The prepottery Poverty Point complex may be coeval with the early phases of upper Mississippi Valley Hopewell and rather closely related to it. This would explain certain divergences which Poverty Point shows from other late preceramic horizons in the southeast, notably those in the valleys of the Tennessee River.

## BIBLIOGRAPHY

Fisk, Harold N.

1944. *Geological Investigation of the Alluvial Valley of the Lower Mississippi River.* War Department, Corps of Engineers, Mississippi River Commission, Vicksburg.

Haag, William G. and Clarence H. Webb

1953. Microblades at Poverty Point Sites. *American Antiquity*, Vol. 18, No. 3, pp. 245-8. Salt Lake City.

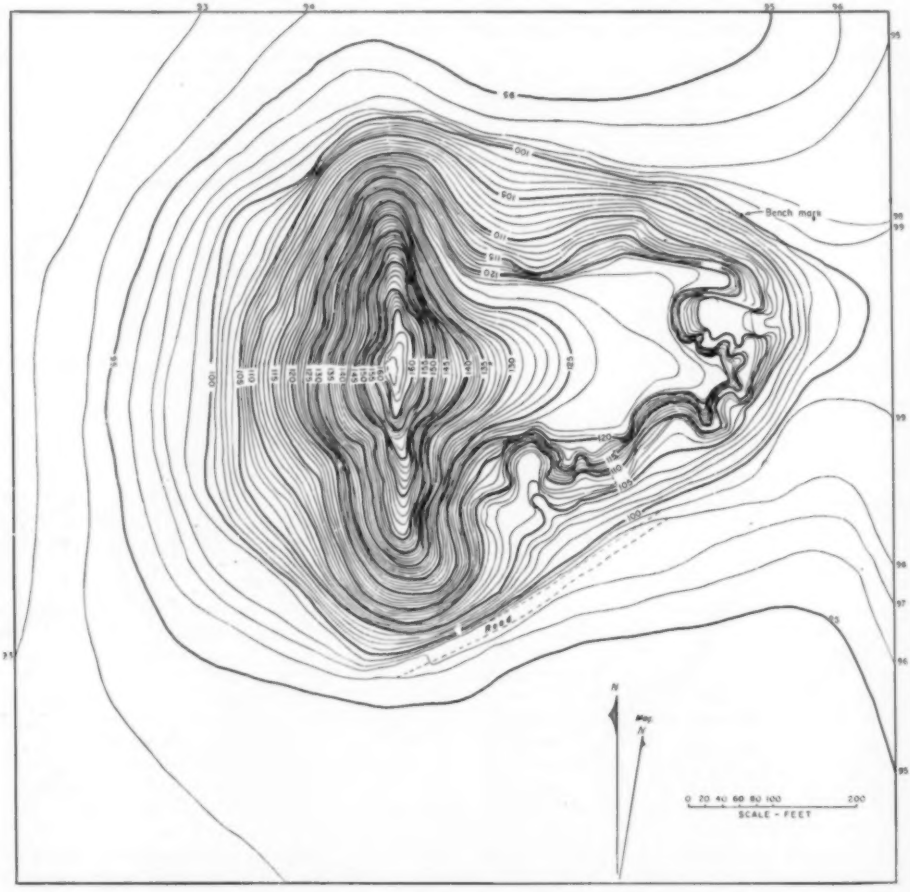



Fig. 74. Contour map of the large mound A at the Poverty Point site. (Datum has an assumed elevation.)

KULP, J. LAWRENCE, HERBERT W. FEELY, AND LANSING E. TYRON
1951. Lamont Natural Radiocarbon Measurements, I. *Science*, Vol. 114, No. 2970, p. 566.

MOORE, CLARENCE B.
1913. Aboriginal Sites in Louisiana and Arkansas. *Journal of the Academy of Natural Sciences of Philadelphia. 2nd series*, Vol. 16, pp. 66-76.

WEBB, C. H.
1944. Stone Vessels from a Northeast Louisiana Site. *American Antiquity*. Vol. 9, No. 4, pp. 386-94.
1948. Evidences of Pre-pottery Cultures in Louisiana. *American Antiquity*, Vol. 13, No. 3, pp. 227-32. Salt Lake City.

JAMES A. FORD
American Museum of Natural History
New York 24, N.Y.
April, 1953

## SITE ON FREMONT ISLAND IN GREAT SALT LAKE

The existence of an enigmatic site on Fremont Island in Great Salt Lake has been brought to the attention of the University of Utah by Earl Stoddard of Ogden, Utah. The archaeological specimens which Stoddard gathered from the site, especially the stone bowls, do not seem to bear any relationship to known cultural materials from northern Utah; consequently the site is being reported here in order to call attention to the unusual assemblage of artifacts. The materials reported and illustrated are in the possession of Stoddard.

FIG. 75. Fremont Island, Great Salt Lake, Utah.

Fremont Island is five miles long, two miles wide, and contains approximately 2,945 acres (Morgan, 1947). It lies twenty-two miles west and south of Ogden, Utah, and only four miles south of Promontory Point (Fig. 75). It is the second largest island in the lake, and the second island to yield definite evidence of aboriginal human occupation; Stansbury Island was the first island on which archaeological materials were found (Jameson, 1948). Stansbury Island differs from Fremont in that the former is not now truly an island. It is situated on the present edge of the lake and is accessible by road. Archaeological materials have been reported from Antelope Island, the largest true island in the current lake, but as yet no survey has been made of the island to determine the extent or significance of reported materials.

Fremont Island is usually completely detached from the mainland except during periods of low lake level. The present (1953) level of Great Salt Lake is 4200.2 feet above sea level and Fremont Island is completely surrounded by water. It is possible to walk to the island dry-shod when the lake level stands at 4194.5 feet or lower; that is, 5.7 feet lower than the present lake level. The most recently recorded low period of the lake was between 1935 and 1940, when the lake dropped to an elevation of 4194.8 feet above sea level. Other lows were recorded in 1905 when the lake stood at 4196 feet and 1860 when it was at 4199.5 feet above sea level.

The site being reported is situated near the southern tip of the island. It lies on the southern slope of a hill which rises gradually to a height of approximately 300 feet above the present lake. Immediately east of the site there is a steep escarpment which drops off to a narrow beach. Several brackish seep springs are located along the base of this escarpment. The site is approximately 1000 feet long and 150 feet wide, and extends down the slope of the hill from its crest and along the edge of the escarpment. The cultural materials at the

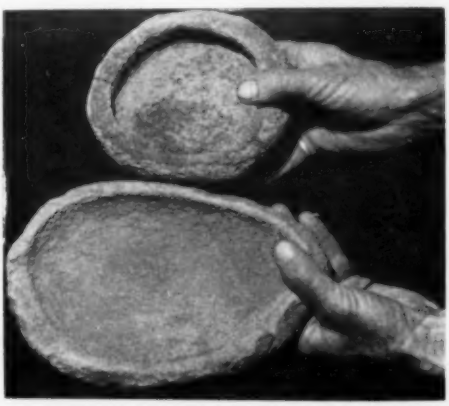


FIG. 76. Fremont Island stone bowls of ferruginous quartzite.

site have been uncovered primarily by wind erosion which has removed several inches of the topsoil. In the areas surrounding the site a scanty grass cover is resisting the erosion and is helping to retain the thin layer of topsoil. A few artifacts have been found in these uneroded areas which indicates that the actual site may cover a much larger area than has yet been exposed. It is quite possible that there are other sites on the island, but none is known at this time. Stoddard has found two manos and several projectile points elsewhere on the island, but no systematic search for other sites has been made on the island.

The list of evidences of occupation on the site include stone bowls and bowl fragments; a variety of objects made from slate, such as blades; needles (?); hooks (?); circular discs, some of which have been perforated; large irregularly-shaped objects; grooved and ungrooved elongated circular stones; thin tabular subrectangular stones, many of which were used as manos; and metates. Except for the obsidian and chalcedony, all lithic materials used for artifacts can be found on the island. The various types of stone used are identified by Norman C. Williams, Department of Geology, University of Utah, as quartz schist, a highly metamorphized slate, sericite schist, and ferruginous quartzite. They are all pre-Cambrian in age.

Although there have been no controlled excavations at the site, Stoddard has made several tests during the past few years. Thin bands of charcoal have been observed a few inches below the present site surface in several of these tests, but there are no indications of hearths or fire pits. One test hole was three feet deep and at the bottom of this hole a thin charcoal and ash band was found. No artifacts, however have been encountered in the tests. Stoddard has found two schist slab-lined pit structures at the site during his diggings. These structures were 2 feet wide, 2½ feet deep, and 4 and 6 feet long respectively. The floors, however, are not paved with slabs, nor is there evidence of any form of wall or roofing above the slabs.

Skeletal remains of two individuals were also uncovered by Stoddard in his testing. The burials were in a very poor state of preservation and only fragments of the crania, the mandibles, and a few ribs were recovered. The crania are so fragmentary that it is not possible to make any observation on them. Tooth erup-

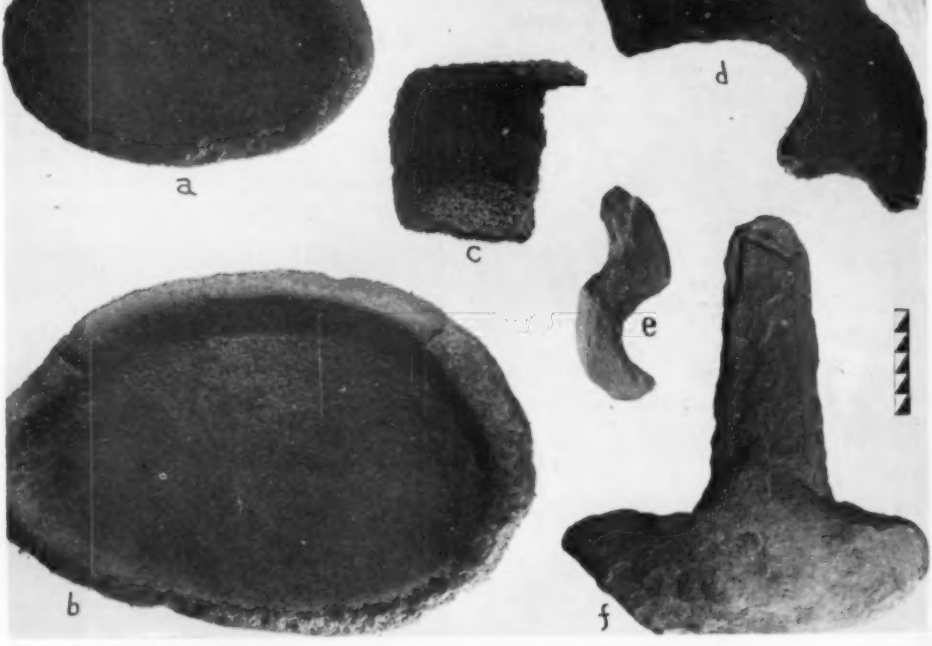



FIG. 77. *a-c*, stone bowls of ferruginous quartzite; *d-f*, irregularly-shaped objects of sericite schist from Fremont Island, Utah. Scale in centimeters.

tions, however, indicate that the burials were those of a child six years of age and of an adolescent approximately 18 years of age.

The assemblage of specimens from the site is of particular interest because it shows very little if any similarity to any of the other sites in northern Utah. The stone bowls, made from ferruginous quartzite, comprise perhaps the most unusual feature of the site. There appears to be no uniformity in size or shape of the bowls. They are oval, circular, and even tend to be rectangular (Figs. 76; 77, *a-c*). A total of six whole specimens and several hundred broken and unfinished specimens have been found by Stoddard. The average thickness of the sides of the bowls is 17.9 mm. ranging from 12.2 to 24 mm. The bottoms of the bowls average 14.7 mm. in thickness, ranging from 10 to 26 mm. The depth of the bowls, measured from a line tangent to the wall rims, is 34.1 mm. with a range from 20 to 55.5 mm. Although there is a great variation in bowl shapes, the average exterior length is 201.2 mm. with a range from 173 to 233 mm. The exterior width averages 161.4 mm. with a range from 116.5 to 201 mm.

The incomplete bowls and fragments which have been found make it possible to determine the technique of manufacture. Manufacture appears to have been a combining of a pecking and crumbling process. The surfaces of the bowls are pitted and quite rough. Although the iron in the ferruginous quartzite will leach out leaving a rough and pitted surface, many of the objects made from this stone still retain evidences of having been abraded and smoothed. The bowls, on the other hand, do not exhibit any good evidence of ever having been so abraded or smoothed. The shape and depth of any given bowl appears to have been determined by the size and shape of the individual stone selected for use. The stones are generally flattish on one or two surfaces, and the surface which was to be the bottom of the completed bowl appears to have been shaped first; generally this was rounded. Then, on the other surface in which the depression was to be made, a circular, oval, or rectangular outline was pecked on the stone which indicated the shape of the completed bowl concavity. This concavity was then pecked out until the desired shape and depth was achieved.

The use to which the bowls were put is not known. The size and odd shapes would make them impractical as metates or mortars, and the surfaces, as pointed out, do not show smoothing which would have resulted if they had been utilized for such implements. Furthermore, no hand tools which could conceivably have been used as hand stones in the bowls have been found at the site. The inner vessel walls on several of the bowls possess slight outward concavity (Fig. 77) which would make it difficult to grind foodstuffs in them.

The only other recorded find of a stone bowl from northern Utah is the one specimen found at Grantsville by Steward (1936, p. 38, Plate 6, *f*). It was recovered from a puebloid mound site; in size and shape it is well within the range of the Fremont Island bowls. The Grantsville bowl was made of sandstone. The stone bowls which bear the closest resemblance to the Fremont Island specimens are the Eskimo stone lamps from Alaska and northern Canada, but there is no evidence at present for postulating a connection between the Eskimo and Fremont Island specimens. The descriptions of the steatite stone bowls and fragments found at the Slick Rock village (Fenenga, 1952), and the reference to basalt bowl fragments from the Chief Joseph Reservoir area, Washington (Osborne, et al, 1952, Table 16), are not detailed enough to permit worth-while comparisons with the bowls reported here.

The obsidian and chalcedony flaked points and blades are characteristic of the site. Figures 78 and 79, *h*, *j*, and *k* illustrate the variety of forms found. Except for two or three forms, the points show very little similarity to types reported elsewhere. The "swallow-tailed" point (Fig. 78, *d*) is almost identical to a point type found by Gebhard in blowout sites in Colorado (Gebhard, 1949, Fig. 48, *d*). The Colorado specimens were found in the marl and lower soil zones in the blowouts which were intermediate between the soil zones containing "Folsomoid" and "Recent" artifacts. Gebhard concludes that the artifacts found in this particular soil zone might be "distinctive points which are not now considered as Yuma [but] were contemporaneous with Yuma points" (Gebhard, 1949, p. 143).

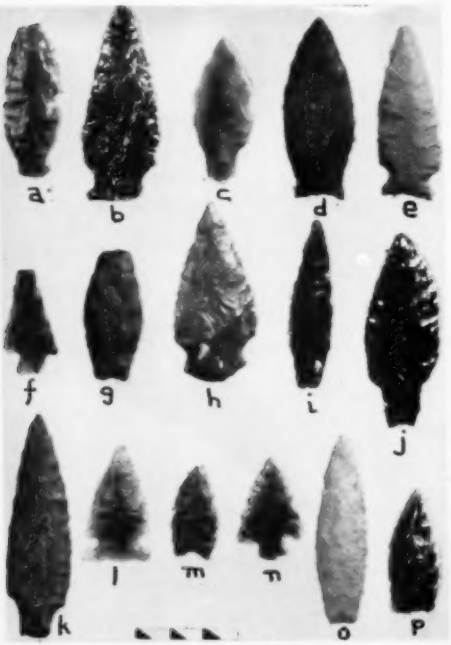



FIG. 78. Projectile points from Fremont Island, Utah. *a-c*, *h-j*, *l-n*, and *p*, obsidian; *d*, *e*, *g*, *k*, and *o*, chalcedony. Scale in centimeters.

Points with leaf-shaped blades, slighthly tapering stems, and concave bases (Fig. 78, *j*), are similar to a point type (Type V) found by Smith in Deadman cave, Utah (Smith, 1952, Fig. 5, *b*, 4 and 5). The stratigraphic position of the Deadman cave specimens suggest that this point type belongs to a prepottery occupation of the cave; that is, they predate the puebloid occupation of the cave.

The points with triangular-shaped blades, rounded to sharp oblique tangs, and straight, convex, and concave bases (Fig. 78, *f*, *h*, *l*, *n*), are very similar to point types found at puebloid sites throughout western Utah (Steward, 1936; Rudy, 1953). It is of interest to note that this particular type point was found primarily in one small area of the site on the island. There were few, if any, objects made from slate found in this particular area.

The objects made from slate are present in a large variety of shapes and forms. Such objects as leaf-shaped blades, possibly knives (Fig. 79, *j*), stemmed blades (*g* and *l*), small needle-and hook-like objects (*a* and *f*), circular discs, drilled and undrilled (*d*), and pendant-like objects (*b* and *m*) were found. The use of slate for such a large variety of objects does not have any direct parallel with any of the known sites in Utah. Steward found etched slate stones and one small slate point in the Promontory Point caves, but there is no mention of any other object made from this type of stone (Steward, 1937, pp. 77-9). Smith found a drilled, rectangular, serrated-edged pendant in Deadman cave which is similar to some of the Fremont Island pendants. The pendant from Deadman cave, however, was made from calcite. It was recovered from level 2 of the cave, which contained puebloid pottery (Smith, 1952, Fig. 13, pp. 23-4).

Sericite schist and ferruginous quartzite were used to make many of the irregularly shaped objects which are unique to this island site. The objects made from the schist are generally flattish, averaging 2 cm. thick, and are S-shaped, T-shaped, notched crescent-shaped (Fig. 77, *d-f*), as well as L-, Y-, and notched triangular-shaped. They range from 9 cm. to over 35 cm. in

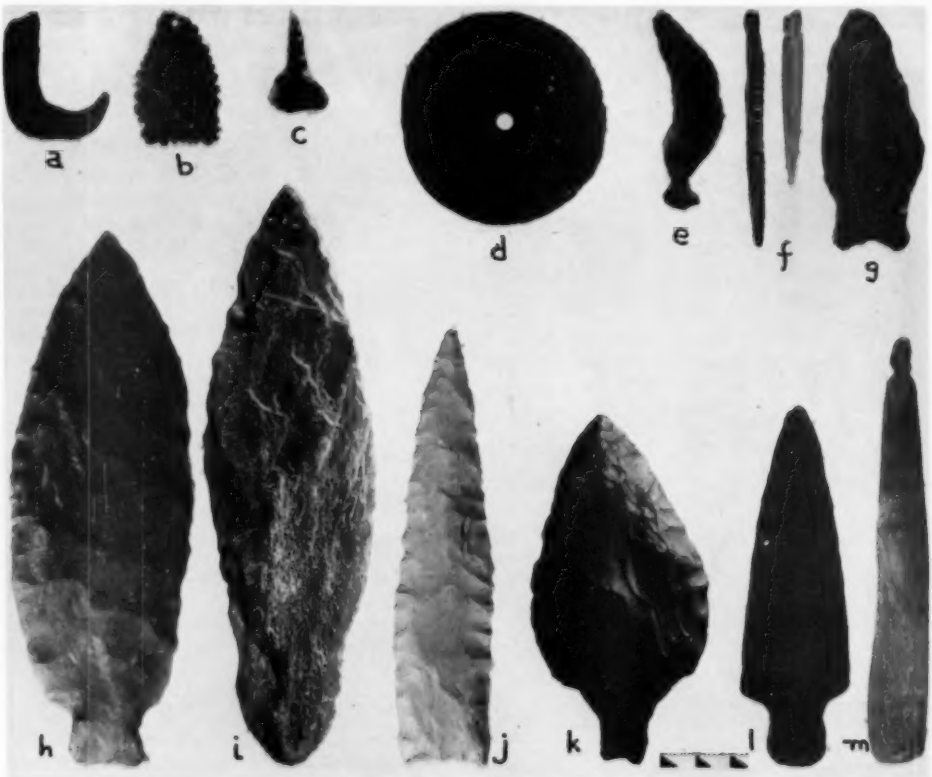



Fig. 79. Artifacts from Fremont Island, Utah. *a*, *b*, *d*, *f*, *m*, slate; *c*, *e*, *k*, obsidian; *g*, *i*, *l*, sericite schist; and *h*, *j*, chalcedony. Scale in centimeters.

length. The edges of these objects have been intentionally chipped, leaving little doubt that they were made by man and are not just fortuitous occurrences of nature.

The use of ferruginous quartzite for stone bowls has already been mentioned, but this stone was also used to make many elongated, rounded, pointed, and blunt-ended objects. Some of these objects have been fully grooved one or more times and may have been used as hammers, picks, or mauls (Fig. 80, *b*). They range in length from 11.5 to 25.5 cm. Phallic symbols were also made from this stone (Fig. 80, *b*). Such symbols have not been previously reported from northern Utah.

A large number of uniformly shaped, subrectangular stones averaging 17.5 cm. in length, 10.9 cm. in width, and 3.1 cm. in thickness were also found. Many of these stones have been smoothed on both flat surfaces suggesting that they were utilized as rubbing stones or manos. Out of the 120 specimens in the Stoddard collection, 90 per cent of them show use on both surfaces. Very few milling stones or metates, on the other hand, were found. The metates are, for the most part, unmodified slabs of ferruginous quartzite with a flat or slightly basined grinding surface.

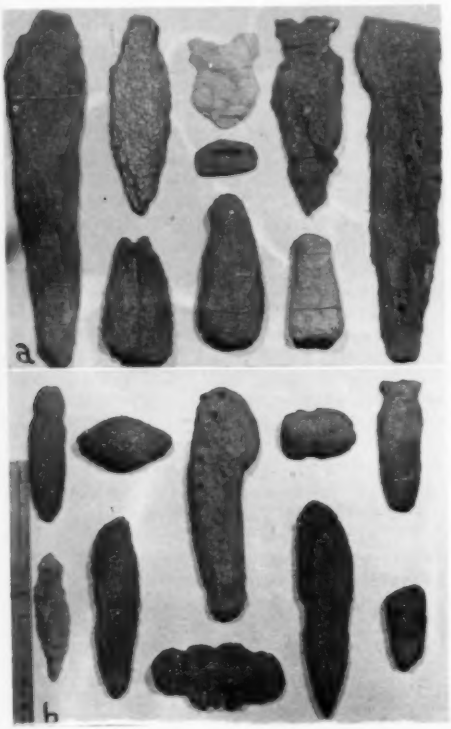



FIG. 80. Irregularly-shaped objects of sericite schist and ferruginous quartzite from Fremont Island, Utah. The first and third objects, top row *b*, are phallic symbols of ferruginous quartzite. Scale same for *a* and *b*.

It is apparent that this site on Fremont Island yields some unique specimens. The occurrence of archaeological materials on the island does not present a problem in itself when it is realized that great fluctuations of Great Salt Lake water level have occurred from time to time and have united the island with the mainland. However, there are no data as to when, in prehistoric periods, the lake was low enough to permit an aboriginal occupation. In view of the fact that all of the artifacts collected are from the surface of the site, there is no way to determine whether the specimens are from one occupation or several.

Our failure to find any pottery at the site, the similarities of two point types to possible preceramic horizons (Smith, 1952, pp. 36-9), and the general lack of resemblance of the specimens to artifacts associated with pottery, all suggest that the site was occupied primarily during the preceramic periods of Utah. However, the absence of pottery does not unquestionably mean that the materials are all preceramic in time because the presence of puebloid-like points argues for at least one occupation by a pottery-making group.

The most important aspects of the site are the stone bowls and the pecked and flaked irregular stone objects which have specifically been reported here for record purposes. The stone bowl found by Steward at Grantsville does not necessarily belong to the puebloid culture found at Grantsville. It is most significant that the bowls and other stone objects have not been found and reported from sites in northern Utah or any of the states bordering Utah.

## BIBLIOGRAPHY

FENENGA, FRANKLIN

1952. The Archaeology of the Slick Rock Village, Tulare County, California. *American Antiquity*, Vol. 17, No. 4, pp. 339-47. Salt Lake City.

GEBHARD, PAUL H.

1949. An Archaeological Survey of the Blowouts of Yuma County, Colorado. *American Antiquity*, Vol. 15, No. 2, pp. 132-43. Menasha.

JAMESON, SYDNEY J. S.

1948. Archaeological Notes on Stansbury Island, Utah. M.S. Thesis, Library, University of Utah. Salt Lake City.

MORGAN, DALE L.

1947. *The Great Salt Lake*. Bobbs-Merrill Company. Indianapolis.

OSBORNE, DOUGLAS, ROBERT CRABTREE, AND ALAN BRYAN

1952. Archaeological Investigations in the Chief Joseph Reservoir. *American Antiquity*, Vol. 17, No. 4, pp. 360-73. Salt Lake City.

RUDY, JACK R

1953. An Archeological Survey of Western Utah. *Anthropological Papers, University of Utah*, No. 12. Salt Lake City.

SMITH, ELMER R.

1952. The Archaeology of Deadman Cave, Utah: A Revision. *Anthropological Papers, University of Utah*, No. 10. Salt Lake City.

STEWARD, JULIAN H.

1936. Pueblo Material Culture of Western Utah. *The University of New Mexico Bulletin*, Anthropologic Series, No. 287. Albuquerque.

1937. Ancient Caves of the Great Salt Lake Region. *Bureau of American Ethnology*, Bulletin 116. Washington, D.C.

JACK R RUDY
Department of Anthropology
University of Utah
Salt Lake City, Utah

EARL STODDARD
Ogden, Utah
April, 1953

## RECENT PUBLICATIONS ON AMERICAN INDIAN PHYSICAL ANTHROPOLOGY

The six papers reviewed by T. D. Stewart in the July 1952 issue of *American Antiquity* (Vol. XVIII, No. 1, pp. 71-72; also reviewed in the *American Journal of Archaeology*, Vol. 56, No. 4, October 1952, pp. 234-236, by E. K. Reed) were given in the symposium on new methods and techniques of interpretation of the physical anthropology of the American Indian constituting the second half of the Viking Fund's Fourth Summer Seminar in Physical Anthropology, August 29 to September 3, 1949 (the first half being devoted to the Australopithecinae of South Africa). Abstracts of all papers were published in the *Yearbook of Physical Anthropology, Vol. 4, 1948*, New York (Viking Fund), 1949. In addition to the six papers presented in full in *Physical Anthropology of the American Indian* (ed. W. S. Laughlin, published by the Viking Fund, New York, 1951), these were: Morris Steggerda, "Anthropometry of the living American Indian"; Charles E. Snow, "The Sequence of Physical Types in North America"; and T. D. McCown, "The sequence of physical types in California." Final discussion, of methods and theoretical considerations, was presided over by Stewart as moderator.

Snow's paper is of the greatest general interest to American archaeologists. His major conclusions or suggestions include: (1) in several areas, probably all, very early groups are of a type with long face, high vault, and usually — not always — very narrow and long head; (2) in both Southwest and Southeast, immigration north from Mexico is indicated; (3) indications of an early Eskimo spread across the northern part of the continent are recognizable in certain New England crania; (4) many American types may well have developed locally, without many of the migrations that have often been postulated. McCown's paper emphasized the surprising homogeneity and stability of physical type in the Stockton-Sacramento area over a long period of time.

None of the papers reprinted from other sources in the *1948 Yearbook* (1949) is concerned with Americanist problems, except E. M. Da Silva, "Absence of the sickling phenomenon of the red blood corpuscles among Brazilian Indians," from *Science* 107 (February 27, 1948), 221-222. In the general summary by Gabriel W. Lasker, "Physical Anthropology in 1948: a record of progress," there is only the following paragraph, pp. 5-6, plus references to American Indian studies under subject headings:

"Several recent papers deal with skeletal funds in the Americas (—— Johanna Faulhaber, "Restos oseos de la Huasteca," *Rev. Mex. de Estudios Antrop.* 10:77-98, 1948-49; Sergio A. Quevedo, "La region craneo-facial en los antiguos pobladores de Calca" *Rev. Secc. Arq. Mus. Nac. Cuzco* 3:5-129, 1947; Milchiades Alejo Vignati, "Contribuciones al conocimiento de la paleopatologia Argentina," *Notas del Museo de la Plata* 12:36-48, 1947). The most important new publication in this area is probably the monograph on Tepexpan man. The skeleton is of a male about 170 cm. tall, which, though estimated to be about 11,000 years old [see critiques and reviews of De Terra *et al.*, *Tepexpan Man*, Viking Publ. 11, New York, 1949, cited in general section of "Notes and News," *American Antiquity*, Vol. 16, No. 3, January 1951, p. 284], is morphologically similar to some skeletons of the Mexican archaic period of the last centuries before Christ. In this connection, Stewart has indicated some limitations in the application of the concept on anatomical dating to American material (— T. D. Stewart, "The Concept of anatomical dating as applied to early man in America," *Bull. Phila. Anthrop. Soc.* 1-4, 1948 [published in full in SWJA 5-1, Albuquerque, Spring 1951, pp. 1-16] ). In the United States, the report on skeletal remains of over 1,200 individuals from Indian Knoll reveals a homogeneous population closely related to other southeastern groups that have left similar shell heaps (— Charles E. Snow, *Indian Knoll Skeletons of Site Oh2, Ohio County, Kentucky*, Univ. Ky. Reports in Anthrop. 4-3, Part 2, 1948 [reviewed by T.D. Stewart in AJPA 7-3, September 1949, pp. 479-481] ). Fenton's review of the skeletal material now available from northeastern North America leads him to suggest that other types may have been in the area contemporaneously with the long-headed type (— pp. 494-496 of William N. Fenton, "The present status of anthropology in Northeastern North America," *Amer. Anthrop.* 50:494-515, 1948)."

A number of 1948 papers dealing with various Central and South American Indian groups are listed in Note 79 (page 9) in the *1948 Yearbook*. Among additional papers cited in connection with various topics, those of interest to North American archaeologists include: Carlos Rusconi, "Pelvis de organizacion primitiva en los indigenas prehispanicos de Mendoza," *Rev. de la Mus. de Hist. Nat. de Mendoza* 2:207-213, 1948; P. O. Pedersen, "Dental investigation of Greenland Eskimos," *Proc. Roy. Med. Soc.* 40:726-732, 1947, and P. O. Pedersen, *The East Greenland Eskimo dentition: numerical variations and anatomy*, Meddelelserom Gronland 142-3, Copenhagen, 1949 (reviewed by Albert A. Dahlberg in AJPA 7-3, September 1949, pp. 477-479; Marcus S. Goldstein, "Dentition of Indian crania from Texas," AJPA 6:63-84; E. S. Gerheim, "Incidence of Rh factor among Indians of the Southwest," *Proc. Soc. Experimental Biol. and Med.* 66:419-420, 1947; Samuel Fastlicht, "Tooth mutilations in pre-Columbia Mexico," *Jnl. Am. Dental Assoc.* 36:315-323, 1948; and T. D. Stewart, "The true

form of the cranial deformity originally described under the name 'tête trilobée,'" *Jnl. Wash. Acad. Sci.* 38:66-72, 1948.

There should also be noted here S. Linné, "Dental decoration in ancient Mexico, a preliminary note on the composition of the cement used for fastening the inlays," *Ethnos* 13:190-193 Stockholm, 1948.

The *Yearbook of Physical Anthropology, 1949 (No. 5)*, New York (Viking Fund), 1951, of which there is a brief review by F. P. Thieme on p. 410 of the *American Anthropologist*, Vol. 53, No. 3 (July-September 1951), has no papers on American Indians among those reprinted. In the general section, "Publications in Physical Anthropology, 1949," Charles I. Shade lists several papers on American subjects which appeared in 1949. Those cited relating to North America north of Mexico include: Norman E. Gabel *A comparative racial study of the Papago*, Univ. of New Mexico Publications in Anthropology 4, Albuquerque; Sherwood Moore, "On the Catlin mark," *American Antiquity* 14:225-226; Erik K. Reed, "The significance of skull deformation in the Southwest," *El Palacio* 56-4, Santa Fe (briefly discussed by E. Davalos Hurtado in BBAA XIII, 1950, II:222, Mexico, 1951); E. T. Adams, "Skeletal remains of man and extinct animals," *Bull. Tex. Arc. and Pal. Soc.* 20, 1949.

The fifth Summer Seminar, reported in the *1949 Yearbook*, was devoted to techniques of physical anthropology, particularly to methods for dating fossils, photography, and statistical methods. The discussion of radiocarbon 14 dating, led by Donald Collier, and that on chemical analysis of fossil bone, presented by T. D. McCown, were concerned in large part with New World materials and problems.

The *1950 Yearbook of Physical Anthropology* (No. 6, ed. G. W. Lasker and J. Lawrence Angel, Wenner-Gren Foundation for Anthropological Research, New York, 1951) contains "A Bibliography and Historical Review of Physical Anthropology in Canada: 1848-1949," by Robert E. Popham, pp. 161-184, but nothing else on American Indian physical anthropology.

In the 1949 issues (Volume 7) of the *American Journal of Physical Anthropology*, papers on aboriginal American race problems include: G. Albin Matson and Helen J. Roberts, "Distribution of the blood groups M-N and Rh types among Eskimos of the Kuskokwim Basin in Western Alaska" (No. 1, March, pp. 109-122); the reviews by Albert A. Dahlberg of Pedersen's *The East Greenland Eskimo dentition*, and by T. D. Stewart of C. E. Snow's *Indian Knoll Skeletons* (No. 3, September); W. C. and Lyle G. Boyd, "The blood groups and types of the Ramah Navajo" (No. 4, December, pp. 569-574).

Also dated 1949 is the volume *Homenaje a Don Luis de Hoyos Sainz* (Graficas Valera, Madrid, 1949, pp. 358), on the occasion of Dr. Hoyos' eightieth birthday, including among others the following papers of special interest: J. Imbelloni, "Reapertura y modernizacion de una discusion secular sobre los dolicocefalos del Peru antiguo," and A. Mendes Correa, "La posicion sistematica de los Amerindios."

In the 1950 issues (Volume 8) of AJPA there is only one paper on a New World subject: J. Auer, "Fingerprints in Eskimos of the northwest territories" (No. 4, December, pp. 485-488); in the 1951 issues (Volume 9) and those in 1952 to date (Volume 10, Nos. 1 and 2, March and June), there are no papers on American Indian topics, though there are several important articles of general interest.

Among papers presented at the 20th Annual Meeting of the AAPA (at the University of Michigan, March 1951) and abstracted in AJPA 9-2, June 1951, however, 10 are on American Indians or Eskimos. Of the most immediate interest to archaeologists are: T. D. Stewart, "Preliminary observations on the human skeletal remains from Conkling Cavern, New Mexico" (— not differing significantly from modern Indians), and C. E. Snow, "Adena skull deformation redefined" (— probably circular head bindings along with the cradleboard). None of the titles presented at the 21st meeting (March 21-23, 1952, at the New York Academy of Sciences) was on an American Indian topic, but Dr. G. L. Jepson as 1952 Wenner-Gren Foundation lecturer showed and explained a motion-picture describing an early bison slaughter site in Wyoming.

Other papers of interest in physical anthropology of aboriginal America published during 1950 and 1951 include: S. Linné, "Dental decoration in ancient Mexico, II," *Ethnos* 15 (Nos. 3, 4) :166-173, Stockholm, 1950; E. Davalos Hurtado, "Datos antropologico-fisicos de la region de Icano de Santiago del Estero, Argentina," *Journal de la Societe des Americanistes* XXXIX:59-71, Paris, 1950 (reviewed by Juan Comas in BBAA XIII, 1950, Mexico, 1951, II:195-196); Dr. Samuel Fastlicht, "Contribucion al estudio del pegamento de las incrustaciones," pp. 158-165 in *Homenaje al Doctor Alfonso Caso*, Mexico, 1951; J. C. McGregor and W. L. Wadlow, "A trephined Indian skull from Illinois?," *American Anthropologist* 53:148-151 (No. 1, January-March), 1951; and W. S. Laughlin, "Blood groups, morphology and population size of the Eskimo," pp. 165-173 in *Origin and Evolution of Man* (Cold Spring Harbor Symposia on Quantitative Biology XV, New York, 1950; actually issued in 1951), a somewhat more detailed and technical treatment of points discussed in the paper by Laughlin in the volume covered above. A review of the Cold Spring Harbor symposium has already appeared in *American Antiquity* (Vol. 18, No. 1, pp. 87-88); it is also reviewed by Marston Bates, briefly, in the *American Anthropologist*, Volume 54, No. 1, January-March 1952, pp. 87-88; by F. Martin Brown in the *Scientific Monthly* for April 1952, pp. 233-236.

Of general importance, though not specifically concerned with American Indian racial history, is a major recently issued general work — the revised and enlarged second edition of M. F. Ashley Montagu, *An Introduction to Physical Anthropology*, Springfield, Illinois (C. C. Thomas), 1951, pp. 590, 377 ills., $8.75.

ERIK K. REED
National Park Service
Santa Fe, New Mexico

# BOOK REVIEWS

*A Study of Archeology.* WALTER W. TAYLOR. Memoir of The American Anthropological Association, No. 69. 1948. 256 pp. $4.00.

## AN APPRAISAL

An attempt to appraise a monograph only five years after publication would sometimes be superfluous — too late to simply review and too soon to view in proper perspective. But in this instance an appraisal seems warranted by the fact that the few printed reviews and comments on *A Study of Archeology* express a striking range of opinions, and are generally at variance with the widespread verbal criticism evoked by this publication. Also, for better or for worse, this study stands as American archaeology's most ambitious self-analysis and most elaborately proposed reorientation. In print, it has been referred to with apparent approval by Carl E. Guthe (Twenty-five Years of Archeology in the Eastern United States. In *Archeology of Eastern United States,* ed. by J. B. Griffin, Univ. of Chicago Press, 1952, pp. 1-12), who cites it as something of an archaeological landmark, and "the most comprehensive statement calling for a critical evaluation of current archeological procedures and a re-orientation of archeological research. . . ." On the other hand, Irving Rouse has recently called Taylor's study an example of "a rigid, all-inclusive program of research," and observed that such a program "would require too much time and money to be practicable under normal circumstances. Moreover, such a program involves questions of value — of the importance of one objective relative to others — on which it would be difficult, if not impossible, to reach agreement." Rouse concludes that, by contrast, a program embracing a variety of related objectives, each considered independently, "seems to be more in accord with present and past practices in archeology." (The Strategy of Culture History. In *Anthropology Today,* ed. by A. L. Kroeber, Univ. of Chicago Press, 1953, pp. 57-76.)

The most detailed refutation of Taylor's study is in a letter by Robert F. Burgh (*American Anthropologist,* Vol. 52, No. 1, 1949, pp. 114-7) which appeared alongside a rejoinder by Taylor (pp. 117-9); reference will be made below to Burgh's criticisms. In sharp contrast to this thorough-going disagreement with Taylor is the enthusiasm expressed in a review by Glyn E. Daniel, who calls the monograph "welcome as a most distinguished, thoughtful and thought-provoking study. . . . A very important book, which should be read carefully, and pondered over by every European prehistorian — and archaeologist." (*Man,* 1951, No. 139, pp. 82-3). But it is in verbal, and generally informal, comments that archaeologists have been most out-spoken concerning *A Study of Archeology,* and it is my impression that such comments have been preponderantly disapproving and rarely favorable. Some comments have been in the nature of outraged resentment that such "disagreeable" things should have been said in print, especially about such respected leaders in the field of archaeology. Others, and perhaps especially those more nearly Taylor's contemporaries, have expressed disappointment that the soundness of some of his comments should be so completely vitiated by association with many highly opinionated views, and the whole decked out in grandiose language and given the implicit authority which derives from publication *in extenso* by the major American anthropological professional group. Even among those whose criticisms of Taylor are moderate or limited, there seems to have been a general feeling that he has presented neither a completely fair analysis of archaeology's past nor a fully satisfactory program for its future.

The remainder of this appraisal will be devoted to trying to see how much justification there is for some of these hostile and unsympathetic reactions, and also for the favorable opinions of such a reviewer as Daniel. To accomplish this it will be necessary to examine what Taylor has to say about archaeology and decide how well his statements fit the situation.

In his first three chapters (comprising Part I) Taylor traces briefly the development of archaeology from its Renaissance beginnings, then defines archaeology as he conceives it, and finally analyzes in detail the work of six American archaeologists, with strong emphasis on their shortcomings. His survey of our discipline's antecedents and growth makes a valuable footnote of 14 pages to Daniel's recently published *A Hundred Years of Archaeology,* stressing American archaeology in contrast to Daniel's preoccupation with the Old World. In particular, Taylor points out the important fact of archaeology's divergent origins, on the one hand stemming from antiquarianism and romanticism, and on the other hand developing out of geology and paleontology, and he observes that we have continued to divide our allegiance, often uncomfortably, among historical, artistic, humanistic, and anthropological endeavors. These observations are well documented and form an effective prologue to subsequent discussions.

In Chapter 2 Taylor enters a more controversial area, namely the problem of how far archaeology is to be identified in aims and methods with history, and how far with anthropology. The nature and meaning of "history" and of "historiography" are explored in painfully great detail, with the conclusion finally reached that archaeology can be equated only with historiography, and then only when archaeologists "interrelate and integrate their cultural information into cultural pictures or contexts" (p. 43). There follows an equally elaborate attempt to define "cultural anthropology," and the conclusion is drawn that "when the archeologist collects his data, constructs his cultural contexts, and on the basis of these contexts proceeds to make a comparative study of the nature and workings of culture in its formal, functional, and/or developmental aspects, then he is 'doing' cultural anthropology and can be considered an anthropologist who works in archeological materials"

(p. 43). Taylor feels that unfortunately such aims are consistently and intentionally ignored by archaeologists, in favor of a highly "improper" insistence on the development of chronological and spatial frameworks, based on culture traits rather than complete "cultural contexts." Taylor largely ignores the fact that these frameworks are an essential basis for further work, and that most of his colleagues are devoting themselves to establishing thoroughly dependable space and time coordinates before proceeding to the even more important and complex problems of "the nature and workings of culture." He fails to recognize that merely the desire to study dynamic aspects of culture will not assure worth-while results; only when the archaeologists' cultural fragments are assembled correctly can he begin to deal with "contexts" as they really were, and not with assemblages of cultural traits that are mere chance results of his own creation. In the succeeding analyses of several archaeologists' work Taylor repeatedly berates them for their concern with "improper" aims, that is, their basic ordering of the data instead of creating with it "cultures" on a higher level of abstraction. He sees the two as somehow incompatible rather than necessarily undertaken in succession.

It should be pointed out here that it becomes apparent as one reads Taylor's study that he is crusading for a cause with all the fervor of the convert, yearning to bring other poor sinners into the fold and save them from their evil ways. One feels that a great but simple answer is being propounded, one that will solve a myriad of complex problems. In addition to this earnestness, there is in Taylor's manner of presenting his arguments a strangely patronizing air, as though it is really only for lack of putting their minds to it that American archaeologists have so bungled their jobs. This attitude is well exemplified in a comment in the Introduction: "The really important thing is to focus the attention of the archeologists upon the nature of their objectives, their practices, and their conceptual tools. This has been tried before, particularly in the fine article by Steward and Setzler (1938), but the effort has rolled off the archeologists like water off a petrified puddle duck" (p. 7). These two aspects of *A Study of Archeology*, its crusading spirit and its patronizing attitude, as well as the extravagance of some of the dogmatic statements they engender, color all but the preliminary historical section of the monograph, and are probably one cause for much of the irritation with which the sentiments expressed have been greeted.

This is particularly unfortunate in Chapter 3, "An Analysis of Americanist Archeology in the United States," because much of what Taylor says is worth saying and is in line with current trends in archaeological interpretation. But it is especially in this chapter that he most frequently smothers in condescension and a demand for nothing less than perfection what might otherwise have been a series of constructive analyses. The first, and longest, analysis is of the work of A. V. Kidder, both in the Southwest and in Middle America. Taylor finds very little of which he can approve, mainly because of a continuing lack in Kidder's publications of syntheses which present all aspects of the life of a prehistoric group, whether Basketmakers, the Indians of Pecos Pueblo, or the Mayas, and also because of the repeated "failure" of publications to present fully enough the details of provenience, materials, dimensions, associations, and so on. Seldom does Taylor temper his criticism with recognition of the fact that Kidder has often set standards far ahead of those of his contemporaries; his "failures" have been only in relation to Taylor's ideals. Carnegie Institution and its entire archaeological program are strongly criticised, on the basis of an evaluation of 79 publications (nearly all prior to 1946). Taylor classifies 50 of them as descriptive, 21 as concerned with epigraphy, the calendar, and chronology, and only 8 as interpretive. He thus demonstrates that "anthropological" problems have been largely ignored by Kidder and his fellow workers, assuming that the "descriptive" works are essentially useless as a basis for later "anthropological" studies. That this is hardly a proper evaluation of them is borne out by the recent appearance of a volume such as Thompson's *Maya Hieroglyphic Writing: Introduction*, which depends to a significant extent on many preceding noninterpretive works. The attack on Kidder is finally concluded (on p. 67) thus:

> Kidder has provided us with some of our finest statements as to the obligations, aims, and potentialities of archeology. But he has been saying one thing, while results have added up to quite another. When Kidder writes theory he often talks historiography and anthropology. When he directs field work and publishes reports, he talks comparative chronicle.

It hardly seems a justifiable procedure to condemn a scholar, in archaeology or in any other field, because his accomplishments fall short of his ambition. In spite of the justice of some of Taylor's specific points, he leaves the wholly false impression that Kidder's work in particular and Carnegie's in general have been largely wasted effort, devoted to trivial purposes, and achieving little that can contribute to anthropology in general.

The next victim of Taylor's analysis is Emil W. Haury. He, too, is found to have been too much concerned with chronology and comparative studies and to have reported too scantily on his findings for them to be useful for the only sort of archaeology which Taylor considers worth while. Essentially the same faults are next found to characterize the work of Frank H. H. Roberts, Jr. The remaining three archaeologists' work is also demonstrated to be equally lacking in proper direction. William S. Webb is said to have showed an early interest in "cultural contexts," only to have his interest later submerged in attempts to produce trait lists suitable for the type of comparisons proposed by the McKern System. In the case of William A. Ritchie, Taylor finds the same preoccupation with trait lists, and the same departure from a previous interest in cultural contexts, synthesis, and interpretation. James B. Griffin, particularly on the basis of *The Fort Ancient Aspect*, is likewise condemned as being interested purely in the placing of culture traits in time and space, and "not

concerned with culture, its nature and dynamics" (p. 89).

At the close of this long chapter Taylor cites briefly a number of recent archaeological publications in which he is able to find merit, either for their purposes or their results. Among these are Vaillant's *Aztecs of Mexico*, Waring and Holder (1945) on the Southern Cult, C. E. Stafford's *Paracas Embroideries*, J. W. Bennett's *Recent Developments in the Functional Interpretation of Archaeological Data*, and Lewis and Kneberg's *Hiwassee Island* (appearing too late for inclusion for more lengthy consideration, but "possibly the best archeological report I have had the pleasure of reading" [p. 9] ).

What finally emerges from this long and critical survey of the work of six prominent archaeologists? Unfortunately, there is little in this survey that could not have been said far more briefly and effectively. The analyses are too often hostile and carping in tone, and concentrate on sins of omission with few constructive criticisms. This is, of course, by no means the first plea that archaeologists make fuller interpretations of their findings in meaningful anthropological terms. Taylor notes that in 1938 Steward and Setzler urged a "functional" approach, and he might also have cited suggestions by Elsie Clews Parsons and others. But Taylor largely ignores such forerunners and preaches his gospel as a lone prophet.

From deploring what archaeologists have left undone, he turns in the second and longer part of his monograph to the need for a basic reorientation of our archaeological goals, the "new" archaeology which he terms "the conjunctive approach." Before the reader reaches any concrete proposals, however, he is confronted with Chapter 4, "A Concept of Culture for Archeology." Burgh, in the letter cited above, comments on this chapter in connection with the "three logical flaws" which he finds in the monograph:

> First, the thread of Taylor's discourse is tangled in rather pretentious nomenclature. . . . A good example of this . . . is the word culture, which he has so desiccated by definition (pp. 97-112) as to make its meaning lifeless elsewhere in the text.

Kroeber and Kluckhohn, in their recent volume on culture (*Peabody Museum Reports*, Vol. 47, No. 1, Cambridge, 1952) quote the three definitions which Taylor presents and comment on them at some length (pp. 68-9). Their criticisms are relatively minor, but they point out the very close similarity between them and a definition presented in 1945 by Kluckhohn and Kelly (The Concept of Culture. In *The Science of Man in the World Crisis*, ed. by R. Linton, Columbia Univ. Press, pp. 78-106). Taylor's definitions seem to me less useful than others already available; nevertheless they reflect a serious attempt to grapple with a problem that is central to all archaeological work but which has often been slighted or entirely ignored. Taylor first considers culture in the "holistic" sense and presents two definitions:

> By culture as a descriptive concept, I mean all those mental constructs or ideas which have been learned or created after birth by an individual. (p. 109)

> By culture as an explanatory concept, I mean all those mental constructs which are used to understand, and to react to, the experimental world of internal and external stimuli. (p. 110)

Then Taylor considers culture in the "partitive" or particular sense, and presents a final definition:

> By a culture, i.e., by culture as a partitive concept, I mean a historically derived system of culture traits which is a more or less separable and cohesive segment of the whole-that-is-culture and whose separate traits tend to be shared by all or by specially designated individuals of a group or "society." (p. 110)

Whether archaeologists will find these definitions useful for their purposes or not, it is important that they are presented as part of the conceptual structure upon which Taylor's program is based. Only a few other archaeologists, such as Ford, Rouse, and Osgood, have defined the exact sense in which they use the term culture, and Taylor is correct in saying that most of us have been far too imprecise about this crucial matter.

As the last step in preparing the groundwork for his program, Taylor discusses in Chapter 5, "The Nature of Archeological Data: Typology and Classification." This is one of the least successful parts of the monograph. It is far too long and involved, and rambles into many irrelevancies and trivialities. More serious than this is the fact that it is largely derived from Krieger, Kluckhohn, and Rouse, each of whom has made important statements on the subject of typology and classification which the reader could more profitably read in their original form. Brew's essay on this topic, in *Alkali Ridge*, is dismissed as "wrong" and "beside the point." Incidental to other questions in this chapter is a criticism of the McKern System, singling out especially its overdependence on trait lists. Although Taylor finds himself in good company in this criticism, he feels it necessary to specifically disparage what Julian Steward and Philip Phillips have written along somewhat similar lines as "hitting at a secondary phenomenon" and "beside the point." Here again we see the patronizing attitude that repeatedly weakens arguments which in themselves may have a certain merit, and claims to originality for ideas which have circulated among archaeologists for at least twenty years.

Taylor's program for the rejuvenation of archaeology is at last presented in detail in the 6th and final chapter, "An Outline of Procedure for the Conjunctive Approach," first in outline form and then in a series of brief examples showing how archaeological data should be studied and reported. The following excerpts from the Introduction is Taylor's only clear definition of his approach:

> . . . The conjunctive approach . . . stands in contradistinction to that which is currently popular with Americanists in the United States and which may be termed the comparative or taxonomic approach. The conjunctive approach takes as its first concern the description of the cultures of human groups. It does this by studying the data which are "Conjunctive: conjoining, connecting, connective; . . . Serving to connect the meaning as well as the construction" (Webster's *International Dictionary*). It is primarily interested in the interrelationships which existed *within* a particular cultural entity, while the comparative approach occupies itself primarily with data which have relationships *outside* the cultural unit and attempts to place the newly discovered material in taxo-

nomic or other association with extra-local phenomena. Although in a way the two are complementary, I believe that the conjunctive approach is basic and offers by far the greatest number of possibilities, while the comparative approach is wasteful, is of very narrow applicability, and is often unreliable even for comparative purposes. (p. 7)

For putting this approach into effect, Taylor presents the following levels or steps by which the archaeologist should proceed, pointing out that in practice one may be working on several levels simultaneously.

A. Problem
B. Data
   1. Collection
   2. Study
   3. Presentation
C. Local Chronology (chronicle)
D. Synthesis and Context (ethnography or historiography)
E. Comparative (ethnology)
   1. Cultural
   2. Chronological
F. Study of Culture, its Nature and Workings (anthropology)

If such an outline does not startle archaeologists as being particularly new or controversial, even less should the various examples of procedure under each heading. Taylor strives to show "how it should be done" and in so doing repeatedly falls far short of his own goals. For example, he presents a tabulation of the subject matter of Mimbres naturalistic pottery decoration, concludes that masculine occupations and interests are portrayed, and therefore suggests the hypothesis that the decoration of the vessels was painted by men. This is meant to show the reader the sort of analyses which archaeologists have failed to make. Instead, as Burgh has suggested, it illustrates the danger of letting pseudologic carry us too far from the data in hand. In several other of Taylor's examples he makes simple and unspectacular trait comparisons under the guise of studying "culture, its nature and workings." These examples are disappointing, since they fail to include any sort of dynamic picture of cultural processes as revealed by the data of archaeology, and they are surprising because it is just such comparative studies that have been repeatedly condemned by Taylor as futile and misleading. One of the most elaborate of his demonstrations, coming under the head of "synthesis and context" is an "interpretation of the growth of . . . two pueblo rooms in site Bc51, in Chaco Canyon, New Mexico" (pp. 175-80). Nowhere is Taylor's writing more confused nor his achievement so far short of his aims. If such an interpretation were seriously presented in an archaeological report certain flaws would be noticed immediately, including the lack of a scale on the map and the cross-section, the absence in the cross-section of a key to explain the significance of the stippled portion, the more serious absence of data on masonry, of fill, and of artifact location that would normally be shown in a detailed cross-section, and finally the inexplicable 3-times vertical exaggeration which renders the cross-section useless as an architectural record. Worse than these elementary matters of technique in reporting the data are the length and complexity of the interpretation, which leaves the reader with no clearer understanding of pueblo culture in general or of this site in particular than he brought to it. Several shrewd inferences from small details of the masonry, the fill, and so on, are made but they mean little without the context of our entire knowledge of Southwestern prehistory, to which no reference is made. The application of such a tedious and unrewarding procedure to a really large and complex site can only be contemplated with horror.

In sum, Taylor's study is an ambitious critique, in which many of archaeology's weakest points are mercilessly exposed, and the "conjunctive approach" is proposed as the necessary reorientation of the discipline. But this "new" approach is in truth merely a reflection and elaboration of a general trend in mid-twentieth century archaeology, namely, a dissatisfaction with the mere accumulation of data, and the desire to use data ultimately for meaningful syntheses and interpretations. Taylor's point of view is far from having the uniqueness he claims for it, and is already exemplified to some extent in the works of such men as Willey, Larco Hoyle, and Braidwood, to mention only a few. For no clearly explained reason Taylor chose to "reduce the scope of the analysis to archeology as it is practiced in the United States upon cultures of the Western Hemisphere. . . ." (p. 6), a limitation that should have been expressed in the title. He thus ignores Grahame Clark and Gordon Childe, as well as others, whose work in the Old World has for many years followed the general lines proposed by Taylor in his "new" approach. The influence of Old World archaeologists, especially the British, on workers in this country has been immense and long-continued, and to treat American archaeology as though it existed in intellectual isolation is ridiculous. Sufficient has already been said in this appraisal about the supercilious attitude that mars so much of it; it is particularly regrettable because it betrays the author into brushing aside the work of such people as Steward and Rouse, in whom he would find considerable support for some of his arguments. Only Kluckhohn's strictures on archaeology's shortcomings are quoted with real enthusiasm, and, indeed, it appears that the essence of Taylor's approach and many of his specific suggestions stem directly from the work of Kluckhohn.

The disappointing impression that *A Study of Archeology* leaves, at least on this reader, is all the more surprising in view of the impressive auspices under which it comes to us. Already accepted in 1943 as a doctoral dissertation at Harvard, it was considered worthy of reworking and elaboration under a Postwar Fellowship in the Humanities, given by the Rockefeller Foundation. Nevertheless the net result is often trifling,

compared to the magnitude of the task. This may be largely because Taylor reserves his constructive proposals for a final chapter, and devotes the bulk of his study to a too-hostile survey of archaeology's errors. But even when finally presenting and exemplifying his panacea, he never illuminates in a convincing manner "the nature of culture and cultural dynamics," which he states to be the archaeologists' proper goal (p. 202). Perhaps this failure to practice what he preaches comes from lack of space for a full-scale demonstration, since it is essential to his program that all aspects of an archaeological report should contribute to each other and that the fullest use of the data will be in the elaborate interrelationships shown within the whole cultural context. Obviously, then, we need from Taylor a thorough application of his "conjunctive approach" before the last word can be said in judgment on it. For several of his brief illustrations of the use of data Taylor draws on "unpublished findings of the [U.S. National] Museum's 1940-1941 Coahuila Expedition, of which . . . [Taylor] was Director" (p. 221). Is it too much to hope that when he reports in full on this expedition we will have an application of the conjunctive approach so that it can be appraised on a firmer basis than promises? As James B. Watson said, in a short review in *Archaeology* (Vol. 2, No. 1, p. 55), "Whether he hits the high mark he sets for himself remains for time and actual testing to tell."

RICHARD B. WOODBURY
Department of Anthropology
Columbia University

*Measurements of Some Prehistoric Design Developments in the Southeastern United States.* JAMES A. FORD. Anthropological Papers of the American Museum of Natural History, Vol. 44, Pt. 3, pp. 309-84, New York, 1952. 23 text figures, 3 tables. $2.00.

This paper was originally prepared for inclusion in Phillips, Ford, and Griffin's "Archaeological Survey in the Lower Mississippi Alluvial Valley, 1940-1947," *Papers of the Peabody Museum of American Archaeology and Ethnology, Harvard University*, Vol. 25, 1951, but was omitted from that publication because Ford was dissatisfied with the original version and Griffin objected "in both detail and principle." Subsequently, Ford expanded it to include the sections of the Gulf coast on either side of the Mississippi Valley, from northeastern Texas on the west to the Florida panhandle on the east.

The paper has two objectives: (1) to align the ceramic chronologies over the area covered and (2) to reconstruct the prehistory of selected elements of ceramic decoration within that area. The first of these objectives is carried out with admirable logic and consistency. Because of the preoccupation of Southeastern archaeologists with the formulation of local ceramic sequences, a relatively large number of sequences is available for alignment although, as Ford points out, the unevenness of their distribution makes it necessary to compare them in terms of areas of varying size and differing degrees of completeness of sequence. Comparison is facilitated by the fact that the local workers have all used the standard Southeastern approach to ceramic typology, and Ford is careful to compensate for variations in this approach. He compares in terms of frequencies of pottery types rather than presences and absences, matching the frequency curves in order to obtain a more precise alignment than would be possible simply by equating ceramic periods.

This procedure appears to be an extension of the one which Ford used to align the five local chronologies in the Phillips, Ford, and Griffin volume. Since a larger area is covered here, the results have to be considered more subjective, as Ford is well aware. He cites a number of complicating factors, e.g., the time required for a type to diffuse from one area to another and the tendency for its frequencies to vary through space as well as time, with the result that similarities among a series of areas do not necessarily indicate exact contemporaneity. If and when the data are available, it would be well to check the results by studying the distributions of other parts of culture, such as mound and projectile-point types, and by the use of other methods of synchronization, such as the identification of trade sherds and correlation with sea-level changes, which ought to have some relationship to the succession of meander patterns in the Mississippi Valley.

In the second part of the paper, Ford deals primarily with the prehistory of eight "ceramic decorative traditions": scattered punctations used as primary elements of design, arrangements of straight incised lines, curvilinear incised designs, motifs developed by roughening the background, applications of the rocker stamping technique, paddle-impressed decorations, red-slipped and painted designs, and engraved designs. In the case of each of these traditions, he has prepared a chart on which the temporal ranges of the principal types in which the tradition is incorporated are shown area by area in the form of "'battleship' frequency curves." Drawings of typical pots are superimposed on the curves to assist the reader in visualizing the types, a feature which, in my opinion, is a considerable improvement over previous charts of this kind. The text consists essentially of a discussion and interpretation of the charts, in which the attempt is made, wherever possible, to trace the origin and spread of each tradition from its place of earliest occurrence and greatest frequency.

It is of interest to note Ford's conclusions with regard to several controversial matters. He suggests that Woodland pottery (in which he includes stamped wares) "may well have diffused across the Bering Strait." Mississippi pottery, on the other hand, "certainly had its roots in the southern part of the Mississippi Valley, when first observed, and may have originated further to the south in Meso-America."

Contrary to Krieger, Ford does not regard the Davis site as the key to the latter diffusion. He considers the site to be contemporaneous with early Plaquemine, which is an even later date than Griffin has attributed

to it. It then follows, of course, that the temple mound complex cannot have been brought into the Southeast by a migration from Mexico to the Davis site, as Krieger thought. Instead, Ford envisions successive diffusion of a series of design concepts from northeastern Mexico; e.g., he suggests that horizontal incised lines and polishing may have come in from Huasteca III during the Troyville period, along with temple mounds and arrowpoints; and that engraving and stepped designs may have spread from Huasteca IV during Plaquemine time.

Caddoan ceramics as a whole are said to contain elements derived from the Southwest, the Plains, and the local, Southeastern area as well as from Mexico. In a separate chapter, Ford attempts to factor out those traits which seem to have originated in the Southeast.

Ford accepts Willey's conclusion that there was a gap between the early and late occurrences of checkstamping on the Florida Gulf Coast and attributes this gap to "the strong surge of plain and incised wares that pushed into the northwest coast from the Lower Mississippi Valley during Santa Rosa–Swift Creek and Weeden Island I times." He would thus appear to reject Sears' contention that checkstamping persisted during the Weeden Island I period, although he does note that there was probably a continuation of this technique "to the east."

As one who has opposed the derivation of Weeden Island ceramics, or elements thereof, from the West Indies, I was pleased to note that Ford traces their origin back through Santa Rosa–Swift Creek into the Marksville pottery of the lower Mississippi Valley. Specifically, he implies that line and dot incision goes back to Basin Bayou and Yokena Incised, contrary to Willey, who has suggested that it may have originated in the Antilles. He similarly considers Marksville Rim Incised to be the source of the custom of limiting designs to the upper part of the vessel wall, which is another West Indian trait. On the other hand, he agrees with Willey that Weeden Island pottery subsequently exerted a strong influence on the ceramics of the lower Mississippi Valley.

"Tacitly or consciously," Ford states in the Conclusions, "present-day archeologists operate on the assumption that culture is a continuum that can, and in their work must be, studied without reference to the human carriers of the phenomena." While there is undoubtedly much truth in this statement, I am unable to accept it completely. Sooner or later, most archaeologists have recourse to theories of migration, which do involve the carriers of culture and may have caused breaks in the cultural continuum of an area; and some archaeologists have even obtained evidence of this. Ford himself mentions migration in one place, suggesting that it may be the reason for the spread of Marksville traits eastward along the Gulf Coast. One wonders whether migration might not also have been responsible for one or more of the irregularities in the frequency distributions, since groups of people may well have moved into an area where the frequencies were different and yet have maintained their old set of frequencies. This, however, would be difficult to determine without a detailed study of the parts of culture other than ceramics.

Nevertheless, it cannot be denied that continuities do exist and are predominant in the archaeological record. In his Preface, Ford asserts his belief, contrary to Walter Taylor's that the future of archaeology lies in tracing these continuities through time and space in order to shed light on the mechanisms of culture change. He offers the paper as an example of what can be done along this line, and it is a good one.

IRVING ROUSE
Yale University
New Haven, Conn.

*Tonala, Mexico. An Archaeological Survey.* EDWIN N. FERDON, JR. Monographs of the School of American Research, No. 16, Santa Fe, New Mexico, 1953.

The title of this monograph is somewhat misleading, in that it is customary to think of an archaeological survey as a reconnaissance of an area of some size. What is presented is more of a survey in the engineering sense, a detailed plan or map of the particular site of Tonala. It is, however, a solid and important addition to our knowledge of Middle American archaeology.

The ruin of Tonala is located on the first ridges of the sierra bordering and overlooking the Pacific coastal plain of southwestern Chiapas and is one of the largest sites in the general area. For many years it has given promise of affording critical information as regards the movements of peoples and cultures through the constricted Isthmus of Tehuantepec region, and no one would question the logic which led those of the School of American Research to initiate work at the site. It is a crossroads location, in the path of movements between Mexico proper and the Pacific coastal regions of Guatemala and further south in Central America and also close to the obvious line of communication between the Lowland Maya and the centers of Mexico proper.

The present study is strictly limited to the architectural and sculptural remains of Tonala based on observations made without the benefit of any excavation. Four detailed plans covering the major building areas are given and these keyed into an over-all map of the site. There are also other detailed plans and elevations of the more interesting or complete buildings and an excellent photograph coverage of architectural details and sculptures. The text contains a listing and description of all buildings and monuments and a general discussion of how this new knowledge of Tonala adjusts and adds to our present conceptions of Mesoamerican history.

Without any internal sequence for the site Ferdon judges that generally an early Classic period date is indicated. This is based on a number of general and some specific similarities to things in Central Mexico, the Lowland Maya, to the Olmec tradition of southern Vera Cruz and to the Cotzumalhuapa complex of coastal Guatemala. The ruin presents a peculiar combination of foreign and unique local elements and, as

Ferdon says, ". . . does not appear to fit comfortably into any of the known regional architectural traditions of the higher prehistoric cultures of Mesoamerica." No outstandingly important trends of influence or migration are apparent, so in many ways Tonala remains somewhat of a mystery.

The study does not, therefore, produce startling results, nor does it exactly clarify any of the major historical problems of Mesoamerica. It stands rather as a well prepared document detailing the principal features of a still potentially important site. The thoughtful care with which the material has been presented leads one to have the utmost confidence in the observational abilities of the author and to consider his work as a solid landmark for a portion of Mesoamerica which has heretofore only been vaguely charted.

One could wish, perhaps, that Ferdon had wavered somewhat from his strictly logical presentation of just the architecture and monuments of his site to say something about the possibility of ceramic studies. He apparently did not have the necessary permission to excavate, but certainly he must have observed either the presence or absence of sherds on the surface and could, within the limits of his permit, have noted such things in his report. Only once is the presence of pottery mentioned in referring to a cache that was reported to have been previously taken from one of the buildings. Some comments on the presence or absence of surface material or of refuse dumps would have been a valuable record for future investigators and would have been of significance in any attempt to judge Tonala as a purely ceremonial versus a ceremonial-living site. But this is a minor criticism of a very valuable piece of work.

GORDON F. EKHOLM
American Museum of
Natural History
New York, N.Y.

*Mogollon Cultural Continuity and Change, the Stratigraphic Analysis of Tularosa and Cordova Caves*. PAUL S. MARTIN, JOHN B. RINALDO, ELAINE BLUHM, HUGH C. CUTLER, ROGER GRANGE, JR. *Fieldiana: Anthropology*, Vol. 40. Chicago Natural History Museum, 1952. Pp. 528, 179 figures, 4 tables.

This invaluable contribution to knowledge of Southwestern prehistory presents the results of the excavation of two long-inhabited caves on the San Francisco River, one of the headwaters of the Gila in west-central New Mexico. Martin and Rinaldo have worked in this area since 1939. Their earlier investigations, published with characteristic promptness and completeness, were of ruins in the open. These had provided much information regarding the nature and the development of local Mogollon culture, but derived entirely from house structures and artifacts not subject to decay. Data were therefore greatly to be desired as to perishable materials, which constitute so much larger a percentage of any people's products, throw so clear a light on the daily life and the industries of their makers, but which very seldom escape the ravages of time. Such materials were especially needed for comparison with those from other caves on the upper Gila and the Hueco area in Texas, admirably described by C. B. Cosgrove, but recovered from deposits less satisfactorily stratified than those worked by Martin and Rinaldo. Together, the two collections are of great importance for judging the nature of the relationship between early Mogollon and other nascent sedentary cultures of the greater Southwest, most of which are best known from specimens from dry caves.

The excavations of the present authors are, as always, carefully described. Superposition of deposits, more orderly than is often the case in such caverns, enabled them to equate the caves' several sequent phases with those of the previously examined surface sites; tradesherds from the caves and the surface sites permitted tying the pottery-making Mogollon phases to several dendrochronologically dated Anasazi phases of the north. The deepest layers in both caves yielded information on a preceramic period. Occupancy, more or less continuous, is thought to have begun some three centuries before the time of Christ, abandonment of the area to have taken place about A.D. 1200. The sections on material culture by Rinaldo, Bluhm, and Grange leave nothing to be desired in accuracy of description and excellence of illustration. Precision of assignment of all types of specimens to stratigraphic level and cultural phase is achieved by tabulated counts and graphs. I can think of no archaeological publication which exceeds and very few which approach this one in usefulness to the student.

The new imperishable finds add to and confirm what was already known of the local cultures. The perishables satisfactorily round out the Mogollon picture. And the very large collection of plant remains, especially of maize, should, when present studies by Cutler have been completed, provide much fresh information bearing on the highly important problem of the origin and spread of Southwestern agriculture.

The happy combination of meticulous analysis and stratigraphic seriation of finds, together with typological and quantitative data, has allowed Martin compactly to sum up the results of the cave and surface site investigations. He sees evidence, as indicated in the paper's title, of both continuity and change; continuity in the long persistence of certain traits, for example round, shallow pit houses; plain brown and red pottery; dependence throughout on agriculture. Change is shown in various ways; appearance of pottery and architecture in the Pine Lawn phase around 150 B.C.; an apparent shift in way of life during the Georgetown phase, estimated to have lasted from A.D. 500-700, when remains of wild plants in the refuse for a time exceeded those of cultivated crops; in maize a sharp reduction in number of rows toward the beginning of the San Francisco phase, that is believed to have lasted from A.D. 700-900; increase in the subsequent Reserve and Tularosa phases, of ceremonialism, perhaps due to influence from the

north, which certainly brought architectural and some ceramic innovations.

Martin feels that although some changes in the local culture were probably due to introduction of elements from without, they were absorbed without serious alteration of a basic and characteristic Mogollon pattern. It is not yet possible confidently to place the origin or trace the routes and methods of diffusion of such extraneous elements. But in view of our present lack of detailed knowledge of the history and the content of cultures which might have contributed and, above all, our ignorance of exact relative chronologies, one could do little more than speculate.

Martin believes that Mogollon, as locally represented by its earliest known ceramic (Pine Lawn) phase, got its start before the first pottery-making Anasazi phases to the north and perhaps those of Hohokam to the west. He may very well be right. He indeed thinks it possible that some not unimportant elements of those cultures may have been of Mogollon origin. He even throws out a suggestion that among these may have been the bow and arrow.

In the concluding section one senses a sort of protective attitude toward the people in whose former homes Martin has labored so long and so profitably. This I can easily understand, for as Haury, Gladwin, Martin, and Rinaldo have knocked the props one by one from under my beloved San Juan Basketmakers as the originators and disseminators of all higher Southwestern cultures, I have found myself acquiring a not dissimilar attitude in regard to them.

To me it seems that the perishable materials from the two caves do little to show Mogollon, even during its early phases, to have been as sharply and fundamentally differentiated from Anasazi or Hohokam as its proponents have held. The matter, of course, is to some extent merely one of nomenclature and as such is of small significance, for what one calls a thing makes relatively little difference if, as obviously do Martin and Rinaldo, one knows and can make clear to others what one is talking about. But of very real significance for a true understanding of Southwestern prehistory is the genetic relationship of these three.

All higher Southwestern developments evidently stemmed from an ancient, widespread, preceramic, hunting-gathering culture. This is well brought out by Martin and emphasized in the section by Roger Grange, Jr. on the wooden artifacts, in which he presents the results of a thorough survey of the literature regarding such materials from regions peripheral to the Southwest. Incidentally, this will save future students of the greater Southwest a vast amount of delving in scattered publications because Grange supplies exact references to provenience, description, and illustration for each type of artifact known from the peripheries.

To me it appears that as time went on this rather generalized hunting-gathering culture became regionally specialized partly as result of an apparently world-wide and age-long tendency for maturing cultures to differentiate, partly because of differences of environment, partly because of varying influences from without. Anasazi, for example, doubtless grew from the above mentioned base culture, became accommodated to a very special arid, mesa-cañon environment and, aside from such early and obviously valuable aquisitions as pottery, maize, and the bow, was singularly resistant to exterior stimuli; it diffused rather than received. Hohokam, with similar origins, was shaped by its game-poor, desert environment which, because of increasing population made possible by the introduction of maize, demanded the development of elaborate systems of irrigation. It received powerful stimuli from Mexico; it radiated relatively little to its northern and eastern neighbors. Mogollon, very possibly the first of the three to acquire pottery and maize, came into being in — to borrow from Toynbee — a less challenging environment. Its response, perhaps for this reason, was relatively weak; its development was sufficient for the satisfactory existence of limited numbers of people but it appears never to have become as clearly and sharply characterized as either Anasazi or Hohokam. Its importance in the over-all picture should not, however, be minimized, for if Mogollon people were indeed the first Southwesterners to have been subject to influences from Formative Mexico; and if they were instrumental in transmitting such traits as pottery-making and maize agriculture to their western and northern neighbors, their role, even if only as middlemen, would have been of great significance. But the question of the extent to which they molded these traits into the forms in which they appear among the Anasazi and Hohokam is one on whose answer must depend final judgment as to their rank as builders of culture. Can we be sure that they, for instance, were responsible for the decorative coiling of pottery, or for the kiva, both brought to a high pitch of development by the Anasazi? And what part did the Hohokam play in the northward and eastward spread of cultural elements? Was the three-quarter grooved axe, which seems to have worked its way east and north from their homeland, and even south into Mexico, a Hohokam invention? The Hohokam apparently transmitted the figurine cult of Formative Mexico to the early western Anasazi. When and where did there originate the dry-grinding of maize? This practise was presumably followed throughout the Southwest, but never, as far as we know, in Mesoamerica or in the wooden mortar-using east. Did dry-grinding get its start among the Cochise for the reduction of wild seeds and continue after the coming of maize? Where, indeed, was maize brought under cultivation? Its most primitive known form seems to be that of Bat Cave in Mogollon territory.

On these and a host of other like questions we must have far more light before it will be possible clearly to understand Southwestern culture history. For this reason we are certainly not yet in position to settle the rather futile controversy as to whether or not Mogollon should be awarded equal rank with Anasazi and Hohokam in the hierarchy of Southwestern cultural complexes. On the basis of most of its material accomplishments, I do not feel that it probably should. But if a

taxonomic arrangement be insisted upon, I incline to follow Erik Reed and class Mogollon and Anasazi as subgroups of Pueblo.

It is tempting to be led from the problem of intra-Southwestern relationships to broader ones of the relationships between the Southwest and the Southeast, on the one hand, and Mexico on the other. The unexpectedly early radiocarbon dates that have been coming out of the East, lead one to ponder. Discussion of such matters, however, would carry one very far afield; nor would it be profitable until we have the chronological data that will eventually be provided by more numerous and more reliable radiocarbon dates, particularly from Mexico. Even in the Southwest, where dendrochronology and cross-finds of pottery have served so well to anchor many cultures and phases in time, there is still need for more chronological information, especially as regards the very early periods. Can we, for example, be certain of the age of the Pioneer Vahki phase of Hohokam, or of Bat Cave maize? The latter's date would, of course, be of special importance in the present connection. Thus one awaits with keen interest Cutler's report on the Tularosa and Cordova preceramic maize. It is to be hoped that many radiocarbon readings may be made from the great number of these cobs available, for it is becoming increasingly evident that single or even a small number of radiocarbon readings cannot implicitly be trusted. Furthermore, readings on groups of these early cave specimens from single levels and squares should provide an excellent test of the radiocarbon method itself, as such lots could reasonably safely be accepted as contemporaneous, at least within the limits of plus or minus error. Dates so established for this and other Southwestern cultures and developmental stages would put us in possession of a sound chronological framework. But for its interpretation in terms of human history we can never do without knowledge of the sort provided by the thorough and conscientiously recorded research of these authors. They have not forgotten that it is the handiwork of men and women, what they made and how they made it, what was treasured and what was cast aside, that brings the past to life, that registers, by improvement or degeneration, man's successes and failures in his unceasing upward struggle.

A. V. Kidder
Peabody Museum
Cambridge, Mass.

*Authors' Comments on Kidder Review*

Dr. Kidder has most thoughtfully sent us a copy of his review of "Mogollon Cultural Continuity and Change" so that we might make comments. Rarely do authors receive such a meticulous reading of their output; and we feel doubly blessed that it was Kidder who judged our work. We appreciate and are in agreement with the criticisms, suggestions, and problems he outlines. One additional point — not sufficiently stressed in our reports — might be brought out at this time.

We have always sensed that the differences between the three subcultures of the Southwest should be based on comparative frequencies — that is, on quantitative differences in popularity of traits and artifact types. It seems fairly clear that the subcultures can not be distinguished alone on mere presence or absence or uniqueness of traits. For example, wickerwork sandals are the most abundant type in early Mogollon periods (300-100 B.C.) and are present, as far as we can tell from published evidence, only in small numbers in early Anasazi (Basketmaker III). By Pueblo III times, this situation was reversed: wickerwork sandals were more popular in Anasazi circles, whereas they had become almost *passé* among the Mogollones.

We have, accordingly, found the differences between the Southwestern subcultures so vast, so complex, and of such a nature that we could place them, for taxonomic purposes, in three categories or groups. On the other hand, we have been astonished to find the counterparts so striking and so numerous that we were able to create our "Southwestern Co-tradition."

On the basis of available evidence, we feel that a closer relationship seems to have existed between Mogollon and Hohokam (before A.D. 700) than between Mogollon and Anasazi. We can not accept, then, Reed's classification that places Anasazi and Mogollon in a Pueblo subgroup, except possibly *after* A.D. 700. Such lumping clouds the issues.

Paul S. Martin
John B. Rinaldo
Chicago Natural History Museum,
Chicago, Ill.

*Preliminary Excavations of Pershing County Caves.* Phil C. Orr. The Nevada State Museum, Department of Archaeology. Bulletin No. 1., Carson City, Nevada. December 1952. Pp. 21., 10 figures.

This is a brief paper reporting the partial excavation of two caves located along the dendritic terrace of ancient Lake Lahontan in Pershing County, western Nevada. Four localities were surveyed and two excavated. The caves in the area have to some extent been damaged by guano miners and by amateur archaeologists.

In Stick cave there was a deposit consisting of a layer of loose guano and rat-nest trash from 3 to 18 inches thick, a hard compact layer of straw, dust, and dung from 6 to 18 inches thick, and below that a layer of tangled masses of grass, reeds, small sticks, dust, rock, ash, and artifacts to a depth of as much as five feet. Fragments of basketry and cordage, two large conical carrying baskets and three large cooking baskets, some smaller baskets, many small dried fish, later identified as a species of chub, remains of larger fish, two small wooden fishhooks (?), fragments of cane, some hardwood points, two obsidian points, and a small effigy of painted granite were found in the cave.

Crypt cave was much larger. It was 60 feet across and 40 feet deep. The deposit consisted of at least four major levels. The top level was recent and was 1 to 10 inches thick. Below was a hard packed layer of dirt, dung, and straw 8 to 10 inches in thickness. Level 3 was of loosely consolidated dust and small bits of rat dung and straw. It was not uniform in depth. Most of the artifacts came from this level. Level 4 was a hard packed layer with a high dung content. Some artifacts and the bone of a native horse were found in this level. Level 5 is the lake bed deposit. A cache of grasshoppers was found buried in the lake sand in this level of the cave. The partial remains of at least eight burials were found in the cave. One burial, that of a female was flexed and wrapped in a finely made net. The body was mummified. Included in the clothing was a robe or blanket of twisted skin, an apron-like garment of string, a fragment of a buckskin skirt, and the whole was wrapped in a large mesh carrying net. A skin bag, a grass brush, a coiled conical basket, and two buckskin moccasins stuffed with grass were also found. The workmanship of the material is very good. Crypt cave seems to have been used intermittently over a long period of time.

No attempt has been made in this preliminary report to give any detailed descriptions of the artifacts or to compare the material with finds from other Nevada caves excavated by M. R. Harrington, S. M. Wheeler, R. F. Heizer, and others. Some geological problems are suggested by these finds of Phil C. Orr which should be given consideration.

This preliminary report has been multilithed. The illustrations are not all as good as they could have been; some are very fuzzy. A detailed drawing of the fishhooks would have been of value. Some type of marked scale would have been better than the small whisk broom used to establish scale in the photograph of the netting from Crypt cave.

This is the first archaeological field venture of The Nevada State Museum. The Directors realize the importance of saving the rich archaeological remains of Nevada and it is hoped that a program of research can be developed. A Nevada State Archaeological Survey would be a worthy cause not only to recover more of the data on the extensive prehistory of the state of Nevada as a local attraction but also in order to place Nevada in the larger context of the Great Basin. It is hoped that financial arrangements can be made to save more of this material before it is beyond saving due to commercial activities and the relic hunting of thoughtless amateurs, and that once recovered the data will be published, to be more readily available to a wider audience of both professionals and others interested in the prehistory of the Americas.

MALCOLM F. FARMER
Museum of Northern Arizona
Flagstaff, Arizona

*Anthropology Today: An Encyclopedic Inventory.* A. L. KROEBER, chairman. University of Chicago Press, 1953. xv+966 pp., including index. $9.00.

*An Appraisal of Anthropology Today.* SOL TAX, LOREN C. EISELEY, IRVING ROUSE, and CARL F. VOEGELIN, editors. University of Chicago Press, 1953. xiv+395 pp., including index. $6.00.

These two volumes are the published result of the Wenner-Gren Foundation International Symposium on Anthropology held at the Foundation's New York headquarters June 9 to 20, 1952 "in order to assess the accomplishments of anthropological science to date and to solicit answers on what direction future research would be likely to take, . . . ." Dr. A. L. Kroeber became president of the symposium and was aided by a seven man planning committee in selecting outstanding anthropologists to prepare the inventory papers present in *Anthropology Today,* and to discuss these papers at the meeting, with special emphasis on future research problems. These discussions are presented in the second volume, *An Appraisal of Anthropology Today.*

There are fifty (50) papers in the 940 pages of *Anthropology Today,* and they present a remarkable coverage from a series of nine papers on applied anthropology through "Primates," "Style," and "Universal Categories of Culture." Archaeologists will have some familiarity with the subject matter of the papers dealing with their particular field. To these they will perhaps give most of their attention, approval, and critical comments. These papers are grouped within the major heading of Problems of the Historical Approach. There are no archaeological papers under the second major heading of the volume, Problems of Process, and understandably so in the third large group on Problems of Application. There are three subdivisions of Problems of the Historical Approach called Method, Results, and Theory.

The papers on Method include "Long-Range Dating in Archeology" by Robert F. Heizer, "Dating Fossil Human Remains" by Kenneth P. Oakley, and "The Strategy of Culture History" by Irving Rouse. Heizer's paper summarizes, under a classification of time reckoning methods, various aids available to archaeologists for dating their cultural remains. These are presented under the headings of astronomical, geological, radioactive, pedological, chemical, geophysical, and accumulation of natural deposits. Additional assistance from the fields of paleontology and paleobotany are also described. This summary has many useful references, and within the framework of the conference it is a usable review for archaeologists.

Oakley's paper dealing with the dating of fossil human remains emphasizes, of course, the older skeletal material which has usually been fossilized and discusses this problem under the heading of various types of relative dating and of absolute dating. He, naturally, pays particular attention to a discussion of the results of dating by fluorine content.

Rouse's paper is organized along the theme of various objectives which are a part of archaeological work.

He presents these as descriptive, classificatory, geographical, chronological, and historic objectives and has a brief discussion on the various means by which different objectives in these major headings may be achieved. The importance of careful consideration of the objectives of a given program is emphasized. It would appear that with the great variety of possible approaches within the archaeological work in an area of any size that it would be quite unusual if all could be studied at the same time under a particular field program.

Under the heading of Results there are five regional summaries of archaeological knowledge. First of these is "Old World Prehistory: Paleolithic" by Hallam L. Movius, Jr., the second is "Old World Prehistory: Neolithic" by V. Gordon Childe, "New World Culture History: South America" by Wendell C. Bennett, "New World Culture History: Middle America" by Alfonso Caso, "New World Culture History: Anglo-America" by Alex D. Krieger. Of these, the papers by Movius and Krieger are the longest of the summaries and, therefore, come more nearly to summarizing the available knowledge of the archaeology. The summaries are particularly valuable because they are the synthesized statements of individuals outstanding in their particular field and with the accompanying bibliography serve as excellent introductions to more comprehensive consideration of the present knowledge of prehistory in the respective areas.

The concluding archaeological papers are to be found in the section on theory with "Archeological Theories and Interpretation: Old World" by J. Grahame D. Clark and "Archeological Theories and Interpretation: New World" by Gordon R. Willey. These will be primarily of interest to professional archaeologists. For Americanists, Willey's systematization of the major objectives of archaeology into "Problem Objectives" and their various levels of interpretive complexity and his comments on current archaeological work will be particularly stimulating.

No attempt is made in this review to give critical statements on the papers presented or on the subsequent discussions, for the subject matter is so diversified within the volumes and this reviewer does not have the omnipotence necessary for such treatment. Presumably it could be said that the contributions are of uneven quality inevitable in such a series of papers prepared by request.

The papers and the discussions are striking examples of the breadth of present day anthropological studies and interests. This breadth, while inherent in the study of man and his works, is also a reflection of the catholicity of the moving spirits in the Wenner-Gren Foundation and their willingness to support and stimulate what seem to be fringe areas of anthropology and other sciences.

James B. Griffin
Museum of Anthropology
University of Michigan
Ann Arbor, Michigan

*Mandan Social and Ceremonial Organization.* Alfred W. Bowers. The University of Chicago Publications in Anthropology, Social Anthropological Series, 1950. Pp. xvi + 407; 44 figures.

This book, as its name implies, is concerned primarily with nonmaterial aspects of Mandan culture. Based upon field studies on the Fort Berthold Reservation made by the author during six months in 1930 and 1931 — apparently supplemented by additional field work in 1929, 1932, and 1933 — it consists largely of descriptions of tribal ceremonies, verbatim narratives relative to these ceremonies by various informants, and accompanying myths. The section on ceremonialism is preceded by sections on historical background, social organization (14 pages), the kinship system, and the Mandan life-cycle and is followed by a brief summary chapter. Four appendices contain additional myths and narratives.

Aside from its general interest to archaeologists because of the light it casts on the culture of a group whose material remains are available for study in documented villages, the book contains occasional statements with implications for material culture which are of more immediate relevance to archaeological studies. For example, "As a little girl, she [a Mandan female] made crude pots when the women of the lodge were making pottery, and, when old enough, she bought her mother's rights in the techniques and designs of pottery making" (p. 62) and "Mandan vessels had a variety of decorations, and the person buying the right to make pottery acquired only the right to employ such decorations as the mother had a right to use. If she wished to utilize other decorations, she was obliged to seek another woman entitled to make the particular decoration and buy the right of her" (pp. 91-92). Another example of material which might be of archaeological significance is the author's description and illustration of eagle-trapping lodges and pits incidental to his presentation of the eagle-trapping ceremony.

Of especial interest to the archaeologist, however, is the thirteen-page chapter entitled "Historic Background." The first — and startling — sentence of this chapter reads "Unlike most tribes of the Northern Plains, the Mandan have moved little since first mentioned in the accounts of travelers, and *their basic culture has changed little except for minor adjustments in their economy resulting from the introduction of the horse and European trade goods*" (italics mine). Historical references to the tribe and pertinent previous ethnological and archaeological studies are briefly discussed, after which the author presents an outline of Mandan history on the Missouri River which apparently began as early as *circa* 1350. If I understand Bowers' exposition — and I am not at all certain I do, since I am unable to reconcile what seem to be two somewhat different versions, one on page 16 and the other on page 18 — the Mandan lived for not more than 150 years in many small villages between the mouth of the Knife River, in North Dakota, and the vicinity of Pierre, South Dakota, during which

time they were characterized by a uniform culture which he has designated the Cannonball focus. By A.D. 1500 the tribe was split into a northern group and a southern group, the cultures of which are distinguishable as the Huff focus and the Lower Grand focus, respectively. By A.D. 1600, the Mandan were all living at or above the Heart River, where they presumably shared a common culture. Bowers makes a number of additional statements about Mandan culture and history of the period antedating written records, and he also accounts for the Hidatsa and Crow after their arrival at the Missouri River. These reconstructions are based on archaeology and traditions: "The archeological evidence collaborates [*sic*] traditions of both the Mandan and the Hidatsa groups relative to migrations and period of residence on the Missouri" (p. 18).

It is difficult to evaluate the archaeology in this book because of its brief and sketchy nature and because of the lack of the supporting data. The propriety of identifying prehistoric cultural complexes with historic tribes is certainly open to question, and a number of Bowers' statements seem to me to be inconsistent with each other or with what we know or suspect to be the case in the cultural history of the area. Archaeologists will be surprised to find that sufficient excavation has been accomplished in this archaeologically complex region to provide such unequivocal and comprehensive answers, in terms of dates and cultural developments, as seem to be implied by the historical summary. They will wish that the two reports by the author (*Archeology of the Upper Missouri* and *A History of the Mandan and Hidatsa*, both on file at the Department of Anthropology, University of Chicago), upon which it is based and which presumably contain the data to support his formulation, might be made generally available through publication.

Paul L. Cooper
1517 O Street
Lincoln 8, Nebraska

# NOTES AND NEWS

## EARLY MAN

The Texas Memorial Museum, Austin, conducted a collecting expedition to the High Plains region of the Texas-New Mexico border in August and September. E. H. Sellards was in charge, assisted by Jerry Ainsworth and Holmes Semken. Oscar Shay of Portales, New Mexico, worked with them. A pure or unit locality was excavated in which square-based projectile points were found associated with bison. These points are similar to those from the upper level of the Clovis site, where they form a component of the Portales complex as described by Sellards in *Early Man in America* (University of Texas Press, 1952), and to those from other localities. The new locality will be described at an early date. A sample of charred bone obtained in a preliminary examination in March, 1953, will be tested for age determination.

George C. O'Neill of Columbia University, working under a fellowship from the Henry L. and Grace Doherty Foundation, reports the finding, on December 31, 1952, of fossilized human remains and crude percussion-flaked basalt implements and projectile points, many with use retouch, at the pueblo of Santa Maria Aztahuacan some 26 kilometers southeast of Mexico City. The fossil remains were found *in situ*, but the associated strata appeared to contain mixed cultural material.

This site was visited during surface reconnaissance of late Pre-Conquest sites in the southern part of the Valley of Mexico. Most of the implements, eventually totaling over 250, were recovered over a period of time from the "spoil" thrown up about the perimeter of a waterhole enlarged by excavation by the people of the pueblo about five years previously. The fossil human remains found imbedded in the south wall of this pit included upper arm bones and vertebrae, *in situ* and in anatomical relationship, which suggested that crania might be found still within the bank. Other fossil bones were found at the same time, 2.5 meters east of these in the same south wall, and later test pits yielded an additional mandible and other fragments.

The discovery was reported to the Dirección de Prehistoria which sent A. Romano, P. Rul, and J. L. Lorenzo to participate for several days in excavating the fossils. This work yielded two crania, upper body bones, two potsherds, and one hammerstone, in direct association. One skull is male, the other possibly female. Both are in a state of excellent preservation and nearly complete. No soil features indicating burial or intrusion were found during excavation. The skulls are highly mineralized and heavy. The male skull is massive with marked supraorbital ridges, sagittal crest, and is slightly long-headed.

Some of the basalt artifacts were found in stratigraphic position in several of the 12 test pits made during the three and a half months of work carried out at the site after the removal of the human fossils. Implements and projectile points of obsidian and chert have been found but are typologically distinct from those of basalt. Points of obsidian, chert, and basalt from the "spoil" around the waterhole are of many forms. In the stratigraphic work in the banks of the waterhole, these artifacts have in every case been associated with ceramics. A transition from preceramic to ceramic levels may be established eventually. The basalt implements possibly will be found to be related to the lowest ceramic levels.

A preliminary sorting of basalt artifacts reveals the following: choppers made from discoidal cores and flakes, hand-hammers, hand-axes, projectile points of various kinds, burins (?), gravers, various scrapers, drills, shaped and retouched knives, and core polishers. Both ground and use-polished stones occur in the "spoil," but as in the other material cannot yet be placed stratigraphically. Bone awls and other implements exist and some appear to be fossilized.

Soil tests are under way to determine the relationships between Santa Maria Aztahuacan and the profiles at Tepexpan and Iztapan (see article by V. P. Sokoloff and J. Luis Lorenzo in *American Antiquity*, July, 1953). Soil analyses as well as Carbon-14 determinations should cast light not only on the absolute dates of strata beneath the surface of Lake Texcoco, but on the rates of bone mineralization under given soil and drainage conditions. The problem of geological dating at the Santa Maria Aztahuacan waterhole is very difficult because, according to local informants, it apparently was at one time an artesian spring.

During the recent deepening of the waterhole by the pueblo, fossil elephant remains were discovered and given to the Museum of Natural History in Mexico City. This digging may have destroyed important evidence for the geological situation, or for the mixing of skeletal and cultural material which may have occurred.

In August, 1953, Krieger inspected the human skulls. They were highly mineralized, heavy, and dark gray in color; the tooth enamel was white but the tooth cores nearly jet black. Arturo Romano, who showed the specimens, stated that the smaller and lighter skull (female ?) was of about the same weight and structure as that of the famous "Tepexpan Man" found by de Terra in 1947 northeast of Mexico City, and that the larger one was definitely more massive and heavier than the Tepexpan skull. At the site proper, it was noted that the stratum in which the human material was found was sealed off by an unbroken horizontal stratum above, with no sign of a pit. If "redeposition" is the explanation for potsherds being associated with the highly mineralized skeletons, it is difficult to see how this could have happened. Much more extensive stratigraphic excavation should be made.

Ruth D. Simpson of the Southwest Museum reports that she is working on the problem of Pleistocene and early post-Pleistocene human occupation of western America. She states that this problem is becoming more complex, with early lithic-industry sites being discovered from Wyoming to Baja California, characterized by arti-

facts typologically simple and lacking in diversity. Choppers, scrapers, and scraper-like tools are typical; core tools predominate; and projectile points are absent. Sites are associated with fossil land forms and, in California's Mohave Desert, with lava outcrops as well as extinct waterways. As more sites are revealed it becomes increasingly apparent that there is marked similarity between specimens from California deserts, the Basin and Range Province, and the Rocky Mountains. Local rocks were used at the different sites.

Typology, quality of flaking, absence of projectile points, and the distinctive topography where sites are found accentuate the belief among investigators of this problem that they are working with lithic industries representing a widespread "Paleolithic" culture horizon. No specific age estimates can yet be made on such sites because the specimens are surface finds — for example, in desert pavements and on lake and river terraces. No cave or stratified deposits have yet been found. Pleistocene association is suggested by the typological simplicity and topographical situations (see *American Antiquity*, Vol. 19, No. 1; pp. 100-1).

## ARCTIC

Dorset and other pre-Thule manifestations were the main interests of archaeologists working in the eastern Arctic during the summer of 1953. In Greenland, Helge Larsen and Jørgen Meldgaard discovered a site in which three separate occupations are separated by sterile layers. In the vicinity of the magnetic pole, Henry B. Collins and William E. Taylor ran across Dorset materials clearly separated from the Thule. In the Churchill region of Hudson Bay, J. L. Giddings reported a flint site which may be pre-Dorset.

A party of four from the Danish National Museum and Aarhus University made a survey of the Disko Bay area in West Greenland with the purpose of locating paleo-Eskimo sites and establishing their chronological position. Of some 60 sites visited 25 contained remains of paleo-Eskimo culture; in addition, 4 sites were found in Umanak district. The majority of the sites belonged to the Sarqaq phase, a West Greenland paleo-Eskimo culture related to early Alaskan cultures as described by Meldgaard (*American Antiquity*, Vol. 17, No. 3). A few contained artifacts resembling those from certain Canadian Dorset finds. In addition to the topographical investigations, more extensive excavations were made in the six foot thick, stratified midden at Sermermiut near Jakobshavn. Here the Sarqaq phase was found at the bottom and separated from it by two sterile layers, a peat and a sand layer, was a peat layer containing Dorset artifacts. Above this was another sterile peat layer terminating in an old grass-covered surface on top of which was a thick midden of Neo-Eskimo culture.

Taylor and Collins conducted further work on Cornwallis Island in the Canadian Arctic for the Smithsonian Institution and National Museum of Canada. Excavating at the site called M2 where they had previously found a few traces of Dorset culture, they uncovered a rather extensive Dorset occupation level, represented by a buried sod line, similar to that which Collins found in 1948 in Frobisher Bay. Material below the old sod was Dorset — chipped stone implements, typical bone and ivory artifacts and large numbers of flint chips. Above this was Thule material. Animal bones from the Dorset level differed from those above in being fragmentary and weatherworn, suggesting that a considerable time interval may have elapsed between the two occupations. Samples of soil, sod, bones, and other organic materials were saved for carbon and other analyses.

The site reported to Giddings by an Indian lies on an old river terrace about 25 miles up the North Knife River, west of Churchill in northern Manitoba. It is within the forest near the tree line, and contains a high proportion of burins and burin spalls, along with carefully executed side blades. This is not a microblade complex, but its components appear, at first glance, intermediate between the Denbigh Flint complex and Dorset culture and more closely related to the former.

Ivar Skarland spent the early summer in the middle Susitna region of southern Alaska conducting an archaeological survey for the National Park Service in a region which may be flooded by a contemplated dam. William Irving continued Skarland's work for the University of Alaska in the late summer in the Tyonek-Lake Louise area where he located several contact sites worthy of excavation. He reports a few prismatic flakes, small diagonally flaked blades, and a number of notched and stemmed points which seem to be unique in Alaska. Also for the University of Alaska, Wendell Oswalt surveyed the Kuskokwim River. He collected about 250 living tree samples as well as samples of archaeological wood and driftwood. He located several middens that will bear excavation, and a flint site at Lake Minchumina.

Wilbert K. Carter and his associates from the Harvard Peabody Museum arrived at Point Barrow on the third of July to begin the 1953 season. Their work consisted largely of continuing the excavations of the previous two seasons. By continued thawing they were able to reach the bottom of more than one impressive mound. Evidence of old Bering Sea culture was found near the base of one excavation, although by the end of the season no true lens or level of this culture had turned up. Mound H at Birnirk reached 11 feet of maximum depth in a trench 186 feet long and from 12 to 18 feet in width. Considerable attention was given to recent house pits including some in which the walls were still standing in order to gain perspective for the older digs. Assisting Carter were Douglas F. Jordan, Henry B. Nicholson, and Thelma B. Carter. They were joined by James A. Ford, who excavated in the Point Barrow sites about 20 years ago, later in July.

A large quantity of selected archaeological wood from these sites is to be studied for tree-ring dating at the University of Pennsylvania and the University of Alaska. Wood samples for possible dating have also been collected by William Irving and Francis A. Riddell in their areas.

## ALEUTIANS

Two parties from the University of Oregon went to work in the Aleutians and on the Alaska Peninsula. James Leach and Wilbur Davis were employed by the National Park Service to conduct archaeological researches in the Katmai Monument, as a part of an over-all study of the natural science of the Katmai Monument and involving several other fields of investigation. Leach and Davis excavated at two village sites on the south coast, Kukak and Kaguyak; and at two sites in the interior, Savonoski and Brooks River. Of special interest is the finding of a site two meters in depth situated at the mouth of Brooks River where it empties into Naknek Lake. Many ground slate knives and points, adze bits and pieces of pottery vessels were recovered.

Philip T. Spaulding, recipient of a grant from the Arctic Institute of North America (with funds provided by the Office of Naval Research) resumed studies in Akutan initiated last summer by the University of Oregon party. Jack Pierce, another graduate student, accompanied him. In the early part of the summer they were able to excavate a site on Akun Island which turned out to be comparatively shallow and recent. They then initiated excavation of a village site on Tigalda, which was known from ethnological information to be the principal village in the Krenitzin Islands. This proved to be deep (5.5 meters), rich in artifacts, and to have preserved wood from top to bottom. For the first time harpoon heads of wood, carved in the same styles as those of bone, were recovered. At the end of the summer when the storms set in, Spaulding and Pierce returned to Akutan village and resumed ethnological and physical anthropological studies.

Theodore P. Bank, II writes from the University of Michigan that his group has "received a grant from the Wenner-Gren Foundation for field work at Unalaska next summer, and plans to take an expedition of six persons. The objectives are an archaeological and ecological survey of the island, with special emphasis to be placed upon two particularly interesting village sites and further pollen studies. We hope to have a zoologist in our party to investigate certain animal populations which have been of special importance to the Aleutians. John Grayson, who has just returned from work in Labrador, will conduct pollen studies and help with the archaeology. The remainder of the party will include an archaeologist and two biologists, but we haven't decided yet just who will go.

"We hope to be able to determine the impact of ecological factors upon prehistoric and historic Aleut culture at Unalaska, and also upon the local biota, and to decide what cultural and natural adaptations have been made in answer to the demands of this particular environment. This will require several seasons of work, but specifically next summer we plan to undertake ecological studies of plant and animal life at Unalaska, pollen and radiocarbon studies of soil-ash profiles, ethnobiological investigations in the nearby villages and archaeological studies at Eider Point and Neveechin mounds. These are a part of the integrated investigations which we have been pursuing in the Islands since 1948. The Eider Point excavation will allow completion of work begun in 1950 and carried over into 1951. Results so far indicate that this mound may be extremely important to an understanding of cultural succession in the eastern Aleutians. It has, for instance, a well marked stratigraphy, including a broad, culturally sterile humus band which may indicate a long period of abandonment between the earliest culture and the succeeding one. The Neveechin site is of particular interest because it's located upon a bluff which may represent an ancient raised beach."

## SOUTHEASTERN ALASKA

An archaeological crew consisting of Kenneth Lane, Robert Anderson, Albert Olson and Donald McGeein continued last season's excavation of a protohistoric Indian village on Knight Island in Yakutat Bay, Alaska, under the supervision of Francis A. Riddell. Funds for the continuance of this work were made available to Frederica deLaguna under a grant-in-aid from the Arctic Institute, and under a Department of Anthropology, University of California grant-in-aid to Riddell.

Work last summer primarily involved the exposing and recording of two plank houses which were encountered at the lowest levels of the site. Fortunately, the houses had been destroyed by fire thus preserving in a carbonized form the floors and sections of the walls of these structures. Although the site is physically stratified there is little or no evidence that there is any important cultural differentiation between the lower levels and the upper. Enough evidence has now been recorded for this site, however, to give a strong clue to its cultural affinities. Riddell who is preparing the material for publication, hesitates to make any statement concerning these affinities at this time.

In addition to the excavation at Knight Island, Kenneth Lane and Riddell spent a week making an archaeological survey in the Dry Bay-Alsek River region and of the lower Akwe River. This exploratory work was done at the end of the season and consequently their work was hampered by bad weather so that they were able to visit but a few sites.

## PLAINS

Richard S. MacNeish, National Museum of Canada, through A. K. Guthe, writes that he has been active in Manitoba; 231 sites have been located, 149 of these by Boyd Wettlaufer who conducted a survey of the province in 1952 for the National Museum. Some of the important sites were excavated by MacNeish during the 1953 season. These include: 1) Cemetery Point, a stratified site; 2) Sturgeon Falls, a prehistoric Cree site; 3) Portage la Prairie Fort, to obtain historic Assiniboine materials; 4) The United Church site.

Altogether some 20,000 specimens from Manitoba sites have been obtained by the National Museum of Canada.

These came from prepottery sites, mounds, prehistoric and historic village sites. As yet only a preliminary analysis has been made of this tremendous amount of material but some preliminary conclusions are worthy of comment. Southern Manitoba seems to divide into three general cultural areas: 1) the Lake Agassiz basin and the woodlands to the east of it; 2) the central Manitoba prairie area, and 3) southwestern Manitoba, which is, in reality, the Plains area. At present the best sequence comes from the easternmost area. The earliest materials are the Angostura points found on the surface in the area east of the Lake Agassiz basin. The next culture, also out of the Lake Agassiz basin, is characterized by small fishtail points and rather small Plainview-like points. This culture, the Lakeshore culture, also has unilateral multibarbed bone points, a variety of scrapers, choppers, and big blades. This prepottery culture is followed by the Larter culture which has a series of corner-notched and side-notched points, and many stone scrapers. In a general way this is like Signal Butte II and Brewerton.

The next culture is middle Woodland and has pottery that in many ways is quite similar to Point Peninsula. This culture is called Lockport, which in turn gradually grades into the Winnipeg focus which is characterized by cord-wrapped stick-decorated pottery, small triangular and side-notched points, and a variety of bone tools. This seems to be early prehistoric Assiniboine. Overlying these materials are those of the historic and prehistoric Cree, which are characterized by fabric impressed pottery and a number of other traits.

This sequence in the east is quite comparable to the one in the central section. Surface collections, however, show that Plainview, Scottsbluff, Eden points, and a Folsom point may eventually show that there are earlier human remains in this area. The earliest cultural remains found in excavation are of the Lakeshore variety. These seem to be overlaid by something like Larter which in turn is overlaid by the Rock Lake cultural remains. Rock Lake has pottery that looks like Late Point Peninsula and is undoubtedly related to the Lockport material further east. Overlying it are remains of the Manitoba focus. This culture is characterized by the building of mounds and cord-wrapped stick pottery. It may very well have developed from the Winnipeg focus further east and may have two phases to it, the latter being historic Assiniboine. Intrusive into the area between early and late focus materials are materials of the Pelican Lake focus. This focus has many resemblances to early Mandan.

The final area in extreme southwestern Manitoba was just barely touched upon in the survey and the excavations. However, it is fairly apparent that nonpottery and prepottery cultures last up until late Woodland times, and as yet not a single sherd from this area could be considered either early or middle Woodland. As is perhaps noticeable, the ceramic data from Manitoba show that the earliest pottery is to the east and even there there is no early Woodland pottery. This may call for a revision of the theory that Asiatic pottery came into the eastern Woodlands via the northwest part of the Woodland zone.

## NORTHEAST

During most of the month of August, William A. Ritchie led a field party from the New York State Science Service, consisting of Peter P. Pratt of Toronto, and Charles A. Denman, in excavating an area on Frontenac Island, Cayuga Lake, N.Y. This area was adjacent to that explored by Ritchie for the Rochester Museum of Arts and Sciences in 1939 and 1940. He writes that "three ten-foot sections were taken down in one inch levels to bedrock, with results confirming the previous published excavations. The Lamoka complex occurred without remainder in the lower horizon, and in combination with scanty Laurentian material at a higher elevation. Top level artifacts were attributable to the early Woodland I period. Charcoal from a number of hearths, unfortunately in this section confined mostly to the Lamoka component, was carefully collected for possible additional radiocarbon dating."

During part of September and October, Ritchie, aided by Irving Rouse of Yale and Peter Pratt, conducted archaeological reconnaissance and excavation on central and eastern Long Island. The full results have not as yet been ascertained, but a significant contribution to the little known Orient focus of the early Woodland period was made by the party, with the volunteer assistance of Douglas S. Byers, at the site dug previously by amateurs at Jamesport. Here an undisturbed portion of a large ceremonial pit was carefully explored, and found to contain caches of "killed" steatite pots, projectile points, and other artifacts, in one instance buried in red ocher. Of primary importance was the obtaining of an adequate charcoal sample for radiocarbon dating.

Also in New York State work has continued on the historic period Mohawk-Caughnawaga site. To date, the Reverend Thomas Grassmann, O.F.M.C. reports that five structures and part of a sixth are outlined.

William Fowler reports the Narragansett Archaeological Society of Rhode Island is excavating a site at Green Point on Narragansett Bay.

Maurice Robbins, of the Bronson Museum in Attleboro, Massachusetts, reports the Cohannet Chapter of the Massachusetts Archaeological Society has been excavating during the past season at an Indian site on Lake Assawompsett in Middleboro, Massachusetts. Although this site is known as that of an historic village indications of earlier prehistoric cultures are present. The better part of the summer was devoted to the excavation of what appears to have been a hollow between sand dunes. This depression seems to have been occupied by at least two cultural groups. The earlier of these two groups left implements suggestive of an Archaic culture including such diagnostic implements as semilunar knives, pebble atlatl weights and long, narrow-bladed, stemmed points. The second culture to occupy the depression is most suggestive of Ritchie's Orient culture from Long Island, New York.

These people dug many pits within the depression. In these broken or "killed" soapstone kettles, grooved gouges and axes, pebble pestles, side-notched and eared points, paint stones, and drilled pendants are found. A most interesting trait found at this level are caches of implements deposited in red paint, another trait which seems to equate with the Orient focus.

According to William Mayer-Oakes, the Upper Ohio Valley Archeological Survey conducted summer excavations with a party of eight for three months at the Watson Farm site in the northern West Virginia panhandle. A small middle Woodland earth and stone burial structure was excavated providing evidence for the associations of the limestone-tempered Watson Cordmarked pottery type. In the adjacent village area excavations revealed a stratified pottery sequence with early Woodland materials underlying the Watson Cordmarked. A small Archaic shell heap was also excavated. Since this was the first systematic excavation of such a unit in the Upper Ohio Valley, analysis of the material should provide new data on preceramic development.

## NORTHERN MISSISSIPPI VALLEY

ILLINOIS. A field party under the direction of Howard D. Winters and John Buettner-Janusch was sponsored by the Illinois State Museum and the University of Chicago. The Barbeau Creek rock shelter near Prairie du Rocher, Illinois, was excavated from June 15 through August 15. Previous excavations had been undertaken by the Illinois State Museum in the summer of 1952 under the direction of Melvin Fowler. The site, which is at least 25½ feet deep, contained principally Archaic material. The upper two feet of the deposit produced a few items indicative of a late Woodland occupation. Excavation was stopped at the 25½ foot level because of an extensive rock fall which could not be removed in the limited time available.

A good series of Archaic artifacts was obtained, and the site will be of considerable importance in interpreting the Archaic period. In particular, strong indications were found of a small tool industry consisting of very small knives, scrapers, and gravers. The projectile point series indicates a need for a re-evaluation of the sequence for the Archaic, particularly insofar as form and size are concerned. Bone and shell were relatively plentiful and well preserved, and a few botanical specimens were obtained. Eight burials were uncovered at various levels in the excavation. Professor Fred Matson of Pennsylvania State College worked at the site on the problem of charcoal recovery techniques under a grant from the Wenner-Gren Foundation. It is hoped that further excavations may be made in the rock shelter next season.

INDIANA. This was a very busy and profitable summer in Indiana. Field school normally held at Angel mounds each summer was conducted as usual under Indiana Historical Society and Indiana University sponsorship. However, the ten week period was devoted to the exploration of a burial site about eight miles east of Angel mounds. This was an emergency excavation in that cultivation and erosion threatened completely to destroy a hilltop site within a short time. Surface observations indicated the presence of stone-lined graves, flesh burials, and cremations. More important was the presence, on the surface, of material evidence of a dual occupation by middle Mississippi and Yankeetown complex folk. Excavation indicated definite priority for Yankeetown as had been anticipated. The burials were poorly preserved and difficult to work out. A considerable quantity of measurable skeletal material was obtained, however, and several interesting burial characteristics were observed. Among other things rather unusual for this area, were drilled long bones and drilled skulls. Material culture was almost completely lacking in the graves making it next to impossible to determine to which of the two complexes the burials belonged. The pottery of middle Mississippi type found at the site differs somewhat from the pottery at Angel and in some respects would seem to be a bit older than Angel.

James Kellar, graduate student in Anthropology at Indiana University, made a site survey of Perry County. This was a continuation of the general site survey program carried on by the Indiana Historical Bureau over the past twenty years. Perry County lies along the Ohio river in southwestern Indiana and, from an environmental standpoint, is somewhat at variance with the counties which lie to the west of it and which had previously been surveyed. Sites in the county were small and there was a notable scarcity of sites which might be attributed to a middle Mississippi occupation. Woodland and, perhaps, Archaic, sites were numerous. The topography is rough and rock shelters were the most common archaeological feature. There were at least fifty large shelters showing evidence of occupation and many much smaller ones. Most of them showed evidence of extensive looting in past years.

J. C. Householder of Indianapolis and Downey Raibourne, graduate student in anthropology at Indiana University, excavated a small portion of a large site near Noblesville in central Indiana. The bulk of the site bears evidence of a Woodland-Fort Ancient occupation and it was hoped that new data might be obtained relative to this duality of occupation so common in this part of Indiana. Much to the surprise of everyone concerned, however, that part of the site which was chosen for excavation proved to be Archaic; Woodland material in scant quantities was found only in the extreme upper levels.

Raibourne also made salvage excavations of some burials in a gravel pit near Indianapolis. Pottery of Woodland type and other materials were recovered with the burials.

MICHIGAN. Two field parties of the University of Michigan worked in the Northeast during the summer. The first was the regular summer field school under the direction of E. F. Greenman. The field school again excavated in the vicinity of Killarney, Ontario, working chiefly on the problem of adequate characterization of

cultures associated with the elevated shorelines of earlier stages of Lake Huron. A summary of the results of the summer's work is not yet available owing to Greenman's absence, but it can be reported that a number of artifacts were found. The second field party consisting of Mr. and Mrs. John Dimick, Albert C. Spaulding, and B. Bruce Powell, conducted a survey of the upper peninsula of Michigan by boat and automobile. Surface collections were made on a number of sites, three sites were explored by test excavations, and several collections were photographed. A major result of the survey was the discovery of an Old Copper site well below the Nipissing beach. The survey was made possible by a generous grant of funds to the University by Mr. and Mrs. Dimick.

MISSOURI. The seventh University of Missouri Archaeological Field Session was conducted this summer in cooperation with Region Three of the National Park Service to salvage archaeological remains from the Table Rock Reservoir Area in southwestern Missouri. The field party, directed by Carl H. Chapman, with Robert T. Bray, a graduate student, as assistant director, also comprised Mrs. Bray, Richard Marshall, Harold Scheffler, James Haswell, Robert Neuman, and Clarence Feik. Most of the research was conducted at the Rice shelter near Viola, Missouri, where tests had been made during the summer of 1952.

The deposits beneath the Rice shelter were completely excavated and a great proportion of the slope in front of the shelter was also excavated. The slope was a midden area built up primarily by preceramic groups to a depth ranging from 4½ to 6 feet. The top few inches of the slope deposit contained debris, including potsherds, of a late middle Mississippi manifestation. Evidences of the latter extended to the bottom of the deposits within the shelter proper (3½ feet). A tentative listing of the cultural complexes found in the shelter and slope area is as follows: (early to late) (1) A preceramic occupation which probably can be divided into two time periods; (2) A preceramic occupation which is probably representative of the Ozark Bluff Dwellers as identified by M. R. Harrington; (3) A late middle Mississippi complex. Inconclusive evidence was also found of other temporary occupations which may be representative of a Woodland (late) and an early middle Mississippi complex (immediately preceding the late middle Mississippi). The three preceramic complexes may be a cultural continuum present over a rather long period of time. Agriculture (or introduction of it) may be the main factor in separating the Ozark Bluff Dwellers from the earlier complexes.

The Rice site excavations, supplemented by tests in four other shelters within a radius of ten miles, seem to furnish a sequence of cultures in the area which can be used as a basic framework for the planning of future investigations in the Table Rock Reservoir area. The other sites tested included the Indian Creek shelter which was completely dry and had not been used as living quarters. It had been used for storage; remnants of nine abandoned storage pits were found. Fragments of coiled baskets, split cane twilled baskets, rope, string and rabbit fur-string were some of the items found in association with the pits. Food items also associated with the pits were gourds, squash, pumpkin, sunflower, corn, acorns, hickory nuts, black walnuts, hazel nuts, wild grapes and wild plums.

The regular field session ended July 31, but a research unit of seven remained in the field until August 28. Several members of the Missouri Archaeological Society aided in the work, Marvin E. Tong, Jr., President of the Ozarks Chapter, being of particular aid. More than 500 people from all parts of southwestern Missouri showed up at the Rice site on Sunday, July 26th, after an "open house" invitation was announced the preceding week. In spite of the large size of the crowd of visitors, it was orderly and there were no casualties among the materials in the excavations.

Robert T. Bray has been added to the University of Missouri Department of Sociology and Anthropology staff as Associate in Archaeological Research (quarter-time) while he continues graduate studies in the Department.

## PACIFIC COAST

William J. Wallace reports the continuation of the agreement between the University of Southern California and the National Park Service for the archaeological survey of Death Valley National Monument. During August, Wallace and his wife surveyed Wildrose Canyon, which is the summer headquarters area for Death Valley National Monument. The region, located in the Panamints at an elevation of between 4,000 and 8,000 feet, yielded only 40 sites and a few artifacts. At the request of Southern California Edison Company, Wallace and his wife spent the early part of September surveying Vermillion Valley reservoir and dam site. Five sites were mapped, one of which had considerable depth. Wallace intends to excavate the deep site later this year.

The Archaeological Survey Association of Southern California published the first issue of its Newsletter in July. Ruth De Ette Simpson is the editor.

Clement W. Meighan, University of California at Los Angeles, with graduate students J. C. Hurst, M. Kowta, and P. H. Kunkel, spent most of August in an archaeological survey of Mono County. Over 300 archaeological sites were recorded. Pottery sites were located 10 miles north of the Inyo County line. This is the northernmost discovery of Owens Valley Brown Ware. The region also contains many aboriginal stone circles (houses or hunting blinds), ranging from 8 to 20 feet in diameter, with walls up to 42 inches in height. J. C. Hurst has been appointed Laboratory Technician and will be in charge of archaeological collections at UCLA.

Meighan also reports that Joseph A. Hester is doing field work on aboriginal agriculture in Yucatan and British Honduras. This is a continuation of research begun in 1952 for the Carnegie Institution.

Charles E. Rozaire, graduate student at UCLA, spent the summer doing curatorial work at the Antelope Valley Indian Museum in Lancaster. The Museum, which has been closed for several years, will be reopened shortly.

The University of California Archaeological Survey carried out a number of archaeological projects during the summer of 1953. Members of the survey continued the reconnaissance and excavated two dry caves in the Central Sierra Nevada in continuation of the work published in the University of California Archaeological Survey Report No. 21, 1953. Working with Washo informants, a number of historic sites were visited and the earlier supposition that the archaeological Kings Beach complex was attributable to Washo authorship was fully sustained. Support for this research came from the Claypool fund of the University of California. Members of the survey also continued site reconnaissance and excavation in Modoc County, northeastern California. In 1952 a survey of the archaeological resources of Lava Beds National Monument was made under contract with the National Park Service. The 1953 work extends, beyond the Monument boundaries, the survey which has thus far recorded some 143 camp, petroglyph, and cave sites. This work was carried out under the direction of R. Squier and G. Grosscup, and supported by the Claypool fund of the University.

Extensive archaeological excavations of two historic sites was performed for the California State Division of Beaches and Parks which contracted with the University of California for the work. J. Bennyhoff and E. Elsasser of the Survey staff were in charge of investigations at Sonoma Mission (founded 1823), and A. Treganza supervised the excavations at Fort Ross State Monument, a Russian stockaded settlement on the Mendocino County coast (founded 1812).

At the Farmington locality, where chipped artifacts are abundant in the early postglacial auriferous gravels (*University of California Archaeological Survey Report No. 22*), J. Davis was in charge of excavation of a trench into the gravel terrace. Artifacts were found down to a depth of 10 feet and the stratigraphy was checked. The most recent opinion is that the Farmington tools may be in the neighborhood of 10,000 years old. What is clear, at least, is that Farmington is the most ancient site of artifacts *in situ* yet discovered in California.

The site reconnaissance of the Colorado River flood plain was continued by M. J. Harner near Parker, Arizona. A total of 23 sites were recorded on land which will soon be leveled for agriculture. Of these, four were nonpottery lithic sites classifiable as Malpais, and were situated on river terraces at the edge of the flood plain. The remaining sites all yielded pottery classifiable as Yuman; a few sherds of non-Yuman pottery in the form of Prescott Grey Ware were found at two sites.

A late horizon site in the Fall River Valley of northeastern California was excavated under the direction of J. Bennyhoff. A fine sample of bone and shell beads and ornaments as well as some pine-nut beads and twined basketry were found with 13 flexed burials. Several types of shell beads indicate trade with northwestern California. The mano and metate appear to have been the dominant grinding tools associated with the bedrock mortar. Fourteen new sites were recorded in nearby Hat Creek.

Archaeological investigations, begun in 1952 in cooperation with the National Park Service, were continued this past summer. J. Bennyhoff was in charge of the testing of two sites with bedrock mortars in the Crane Flat area. Two successive periods were represented at both sites. The upper levels yielded small obsidian points diagnostic of the late horizon. The bedrock mortars can be associated with the late culture because of their known use by the historic Miwok Indians who occupied the area. The lower levels of both sites yielded large points of obsidian (rarely chert or basalt) and several chert and basalt core tools. The abundance of manos and several slab metates from the lower levels suggests that these grinding tools preceded the use of the bedrock mortars. The point types suggest a general correlation with the Middle horizon period of the Sacramento Valley.

Luther Cressman reports that the Department of Anthropology of the University of Oregon cooperated with the National Park Service during the field session of 1953 in excavation of a site just south of Five-Mile rapids at the Dalles damsite reservoir. This site is nearly 700 feet long and 250 feet wide at the point of greatest width. Construction and transportation facilities have destroyed or covered large areas of the site. A series of stratipits was used to sample the area. Several thousand stone artifacts were recovered, and though the material is still being analyzed, there is a strong suggestion of change in instrument types from early to late. Occupation extended beyond a depth of 16 feet in one pit where excavations were halted because of the danger to the workers. Cressman believes that his excavations indicate that cremation is late in this area. Cremation burials were superimposed on burials which can be dated as late as 1820, probably later. A report on the petroglyphs of the Dalles Reservoir was completed by David L. Cole and Jack R. Hegrenes, Jr., graduate students at the University of Oregon. The field work upon which this report is based was carried out during the winter of 1952-53 and was supported by National Park Service funds.

An archaeological and ethnological survey of Katmai National Monument in Alaska was conducted by Wilber A. Davis and James W. Leach, graduate students of the University of Oregon who were employed by the National Park Service and worked under the supervision of William S. Laughlin.

During the summer of 1953, a number of archaeological projects were carried out by University of Washington field parties under the general direction of Douglas Osborne. Warren Caldwell supervised the excavation of the Wakemap Mound in the Dalles Reservoir project. Three large trenches were cut into the mound. Osborne reports that it appears that the Wake-

map site will prove to be the most significant site that has been excavated so far by a University of Washington crew. Much difficulty was encountered with local collectors and the excavation has been facetiously called the "battle of Wakemap." The work was supported by funds provided by a contract between the National Park Service and the University of Washington.

Earl Swanson was in charge of excavations at several sites in the middle Columbia River area near Vantage Ferry. Swanson's crew excavated one cave, tested two others, surveyed intensively a large area, and located and tested a cultural deposit of undoubted antiquity in the Columbia floodplain. Osborne and Swanson are of the opinion that the material from the latter site has affiliation with paleo-Indian cultures. Funds for this work were supplied by the American Philosophical Society and the University of Washington.

Alan Bryan intensively surveyed parts of the Puget Sound area, concentrating on Whidby Island where several test excavations were conducted. Miss Sally Snyder worked with Bryan, collecting ethnographic information. During part of August and September Bryan led a small crew in continuing the excavations carried on in previous years by Louis Caywood of the National Park Service at the Fort Spokane site in eastern Washington. Work at Spokane was supported by the State Parks and Recreation Commission.

Catherine McClellan spent the summer in New York State working with Frederica deLaguna in writing up the results of her research in the Tlingit area.

Charles E. Borden of the University of British Columbia spent the past summer in Europe gathering materials for a new course on Old World Prehistory and for the advancement of archaeological research at that institution. He visited eleven countries, studying and taking color slides of collections, visiting and photographing numerous ancient sites, and conferring with men actively engaged in prehistoric research in Europe. Borden expresses the hope that with the assistance of some of his students he will be able to make progress on the site survey of southern British Columbia begun last year.

Richard D. Daugherty of the State College of Washington completed the final report on the "Archaeology of the Lind Coulee Site." The report covers the results of two seasons of excavations supported by grants and assistance from the National Park Service, the State College of Washington, the Smithsonian Institution River Basin Surveys, and the University of Washington. The report also includes appendices by Charles D. Campbell, Harold E. Culver, and Lee G. Nering on the geology of the site, and by Betty Enbysk on the paleontology.

A crew of five students under Daugherty's direction spent the summer excavating a large cave near the mouth of the Palouse River, a tributary to the Snake River in southeastern Washington. It was found that the cave had been used primarily as a cache for food. Numerous mat-lined storage pits were observed and a number excavated. Large quantities of matting and cordage and some fragmentary baskets were recovered. The material appears to be attributable to a recent cultural phase.

## SOUTHWEST

The Pecos Conference, annual gathering of Southwesternists, met at the Museum of Northern Arizona, Flagstaff, on August 17-19, 1953. Over 100 anthropologists, many accompanied by their families, were in attendance. Harold S. Colton, Walter W. Taylor, and Verne Ray were the interim committee in charge of the meeting.

One session was devoted to reports of archaeological field work of the past summer, and another to ethnological work including several reports by men working on various Indian land claims problems. At succeeding sessions several symposia, planned by Walter Taylor, were held. These included "Origins and Dispersals of Agriculture in the Southwest," which was discussed by Carl Sauer, University of California, George Carter, Johns Hopkins University, and Hugh Cutler, Missouri Botanical Garden; "A Temporal-Spatial Synthesis of Southwestern Culture Horizons," based on the report of a committee appointed in 1952, composed of Harold S. Colton, Edward B. Danson, and Fred Wendorf; and "Cultural and Chronological Relations of the Hohokam-Sinagua-Salado," which was the subject of a lively session led by Albert H. Schroeder, Harold S. Colton, and Joe Ben Wheat. A workshop on "Projectile Point Terminology," under the chairmanship of Herbert Dick, was held one evening.

The conference accepted an invitation from the Southwestern National Monuments to hold the 1954 meeting at Gila Pueblo, Globe, Arizona. Dale S. King, Thomas Onstott, and Richard Woodbury were appointed the interim committee.

Resolutions on the deaths during the past year of two members of the conference, namely Paul Reiter of the University of New Mexico and Tully H. Thomas of Concho, Arizona, were adopted. A resolution of thanks to the El Paso Natural Gas Company for their splendid cooperation in matters of archaeological salvage along the pipe line right-of-way in New Mexico and Arizona was framed.

Joe Ben Wheat, recently of the Department of Anthropology at the University of Arizona and of the Arizona State Museum, has accepted the position of Curator of Anthropology, with the rank of Assistant Professor, at the University of Colorado Museum. He will have active charge of the archaeological and anthropological collections of the University of Colorado Museum, will direct the development of exhibits in the museum, and will engage in excavation, research, and publication on the archaeology of Colorado.

## SOUTH AMERICA

CHILE. Richard P. Schaedel, whose studies of architecture and settlement patterns on the northern and central coasts of Peru have been reported in this department in earlier issues, was in Chile from January to July, 1953, dividing his time between field work and

teaching. For the first five months of the year, he was Visiting Professor of Cultural Anthropology at the University of Chile under a grant from the U. S. State Department, working there in close collaboration with Ismael Silva Fuenzalida who recently completed his doctorate at Northwestern University. In February and March, Schaedel and Silva took a group of four students who had been selected by Dra. Greta Mostny into the field for a reconnaissance of northern Chile. Schaedel reports the following results:

"On the trip, the expedition was able to find more extensive remains of late agricultural settlement in the Azapa, Lluta and Vitor valleys than Bird's investigations had indicated. Tie-ups with Rydén's post-Decadent Tiahuanaco period in the Bolivian highlands were also found. A survey of sites in the maritime zone between Iquique and Antofagasta confirms the cultural sequence which Bird established north and south of this area, at Pisagua and Taltal. Some slight Tiahuanacoid influence was also noted here.

"Data on burials in the Iquique area in general is being carefully recorded by Anker Nielsen of Iquique, and it is highly desirable that his well-documented finds be published, as they will amplify the several phases marked out by Bird for this basically maritime culture of the far northern Chilean coast.

"The expedition stopped briefly in La Serena to review a series of sites of the El Molle culture with its discoverer, Sr. Francisco L. Cornely, Director of the Archaeological Museum of La Serena, and had the pleasure of excavating a rare El Molle burial in his company."

During the months of April and May, Schaedel taught a short course in general anthropology, attended by a selected group of students at the University of Chile. This course stimulated such interest that it was possible to plan the establishment of a permanent Section of Anthropology at the University (Sección de Antropología Social e Histórica). The new Section is under the directorship of Silva Fuenzalida. Leopoldo Pizarro, Gustavo Peña, Pablo Garrido, and other Chilean scholars interested in anthropology are collaborating in its teaching program. The University requested the State Department to extend Schaedel's grant for an additional period to work with the new Section, and Schaedel expects to return to Santiago in January, 1954, for this purpose.

In June and July, another field trip was arranged with the students who had taken Schaedel's course. Schaedel reports: "The coastal region between Algarrobo and Cartagena was selected, since local collections indicated the existence there of a culture similar to that of El Molle and possibly earlier than the 'Aconcagua' occupation which is characterized by polychrome ceramics. Work on this problem will be resumed in January. A few sites were visited which confirm Bird's contention that the Shell Fishhook culture extended this far south. No Diaguite site unassociated with Inca was found in the limited reconnaissance made."

PERU. Victor W. Von Hagen has organized a project known as "The Inca Highway Expedition" for the purpose of locating and studying as much as possible of the famous road system built by the Incas. Earlier travelers have reported seeing many fragments of the Inca roads and remains of the associated tambos (way stations) and bridges, and there is no reason to doubt the feasibility of tracing the major part of the road system on the ground. The expedition is operating under the sponsorship of the American Geographical Society and expects to be in the field from 1953 to 1955.

Most of the preliminary historical research on the Inca roads was done by Dorothy Riddell at the University of California (Berkeley), and she was able to furnish the general routes of the two main Royal Roads, one running up the Andean highlands from Bolivia through Cuzco to Quito and the other following the coast.

The actual field work began on March 6, 1953, with an air and ground survey of the road remains on the north coast. The central and south coasts were then covered, and the expedition turned inland to Arequipa and Puno in May. By August 20 it had reached the crossing of the Apurimac River, north of Cuzco. Von Hagen plans to devote the rainy season (December to April) to further work on the coast road, saving the better weather for the highland survey.

On the coast, the expedition traced the Inca road from Saña to Casma in the north and from Cañete to Chala in the south. There are especially well preserved sections between Chimbote and Nepeña and between Nazca and Chala, complete with tambos and other features. On the central coast, between Huaura and Cañete, the road is almost obliterated and very few fragments of it could be found. Lateral roads leading into the mountains were noted at Nepeña, Huaura, Cañete, Pisco, Acarí, and Yauca.

The coastal road actually lies some distance back from the shore, up to fifty miles in places where the shore is particularly hilly. It is built at a standard width of 23 ft. 9 in. (probably four Inca fathoms); this statement is based on over 100 measurements made by Von Hagen's expedition. The laterals running into the mountains maintain this width on level land, but are narrower in broken country. The chief archaeological problem on the coast is to distinguish Inca laterals from remains of earlier roads, which are fairly abundant locally on the north coast. The main road is comparatively easy to identify.

No road remains were found connecting Arequipa with the main coast road, confirming the impression given by the Inca chroniclers that the Arequipa area was comparatively unimportant in the Inca Empire.

Around Lake Titicaca the road was badly destroyed, but the expedition picked up fragments of it in the hills back of Puno, west of Pomata, and in the village of Zépita. Going north toward Cuzco, the junction at Ayaviri was found, where a branch came in from the east side of the lake. The tambos of Chungara (near

Santa Rosa), Lurucache, and Cangalla (near Checacupe) were located. The road followed the east bank of the Vilcanota River to Quiquijana, then crossed it and followed the west bank to the turnoff for Cuzco. The longest stretch of well preserved road was found between Checacupe and Cusipata where the road is fifteen feet wide, lined with low walls, and paved with stones.

From Sicuani a side road led east to Macusani and a series of towns in the gold producing province of Carabaya. Beyond Macusani it went down the Tambillo River to a tambo near the town of Tambillo; here a branch led southeast to Pucuta. The main road turned northeast to Ituata and Ayapata. Other towns connected by Inca roads in this area are Corani, Tantamaco, Coasa, and Usicayos, all near the 13,000 ft. contour line in the headwaters of the Inambari drainage. There is an extremely well preserved section of the Inca road near Tambillo, with a stone slab bridge and a stairway of 2200 steps climbing a hill 1500 feet high. Remains of the villages and burials in caves with Late Inca pottery are common in this part of Carabaya.

Near Cuzco the expedition traced the road from Pisac to Paucartambo (the Royal Road of Antisuyu), and found another road, not reported by the chroniclers, leading from Calca to Lares. Explorations were made around Machu Picchu and in the Pampaconas valley. No trace of a road connecting Machu Picchu with the last Inca capital at Vilcabamba was found. A small Inca ruin called Puncuyoc, not hitherto reported, was found in the Pampaconas valley.

North of Cuzco, the Royal Road was traced to Izcuchaca, where it crosses the Plain of Anta on a famous causeway to Zúrite. Near the latter place, extensive Late Inca ruins were found which may be the remains of ancient Xaquixahuana. Thence the road was traced over the Pass of Vilcaconga, by Limatambo and Mollepata, and down to the site of the famous hanging bridge over the Apurimac. Extensive ruins were found at Marcahuasi, just below the road, perhaps the remains of a post to guard the flank of the bridge. There are buildings, aqueducts, and terraces here.

The Inca bridge over the Apurimac was maintained and used until about 1880 when a new bridge (Tablachaca) was constructed further south. Markham and Squier give impressive accounts of the earlier one. On the east side the road and abutment are badly destroyed, but on the west side the caves in which the suspension cables were anchored can still be seen, as well as the steps cut in soft rock which lead up the hill above. From the bridge the road was traced to Curahuasi, where the local people gave the traditional location of the Inca tambo north of the modern town; the ruins of it were destroyed a few years ago.

The most serious problem facing the expedition in its highland survey is to distinguish between Inca and colonial roads. The colonial roads followed Inca ways for the most part, so it makes little difference from the point of view of route, but care must be taken to distinguish colonial pavement and tambo construction from the original Inca ones. In a survey as extensive as the present project, the characteristics of Inca road construction should become clear enough so that the distinction can be made without great difficulty.

## THE PILTDOWN HOAX *

In a brief paper, which is a model of clarity, Weiner, Oakley and Clark (1953) present evidence which shows conclusively that the jaw and canine tooth of Piltdown "man" are those of a modern ape, deliberately faked to simulate the fossil skull. Thus the Piltdown problem is solved, and the numerous scientists who maintained that the jaw did not belong to the skull have been proved correct. Miller's (1915) classic demonstration that the jaw was that of an ape deserves rereading in the light of recent events. His argument would have been accepted, except that an early Pleistocene ape in England seemed so improbable. Miller was so close to the truth that one wonders if he might not have solved the problem if his study had been based on the original specimens rather than casts. In the introduction to his 1915 paper he states, "Deliberate malice could hardly have been more successful than the hazards of deposition in so breaking the fossils as to give free scope to individual judgment in fitting the parts together."

Both because of its peculiar anatomy and because it was one of the key specimens used to support the theory that Homo sapiens had an early origin distinct from Neanderthal man, Piltdown has been the center of continuous controversy. Vallois (Boule & Vallois 1952: 178) estimates that nearly 250 papers and monographs have been devoted to it, and Miller cited nearly 70 publications in 1915! In spite of this vast labor, in 1950 the Piltdown problem was in the same state as at the time of the origin of the controversy. Since that time the solution has come rapidly by the application of techniques which settle the problems of dating and association. Oakley and Hoskins (1950), using the fluorine test, showed that the Piltdown remains were of Upper Pleistocene age (provisionally assigned to the last interglacial) and that "all the specimens of Eoanthropos, including the remains of the second skull found two miles away, are contemporaneous." This conclusion apparently eliminated the argument that the jaw could belong to an ape. It had been hard enough to think that an ape might have been associated with the earliest fauna but to suppose that apes were living side by side with beavers in the Upper Pleistocene was utterly fantastic. The late dating made the anatomical problem more difficult too. There are so few early human fossils that the existence of a peculiar kind of man might be admitted, but a very considerable number of men are known from the Upper Pleistocene and not one has a jaw like Piltdown. Dr. J. S. Weiner saw the impossibilities in both directions and suggested to Drs. Oakley and Le Gros Clark that there had been a deliberate fake. Weiner demonstrated that artificial abrasion and

* Reprinted from *American Anthropologist*, Vol. 55, No. 5, Part 1, 1953, pp. 759-62.

staining of chimpanzee teeth produced a result "astonishingly similar to the Piltdown molars and canine."

Restudy of the Piltdown remains revealed an "extraordinarily skillful" and "entirely unscrupulous" hoax. The evidence presented by Weiner, Oakley, and Clark (1953) proving that Piltdown is a fake may be briefly summarized as follows.

TABLE I.

GROUP 1

Modern Bone. Little flourine. Lots of organic matter.

| | % fluorine | % nitrogen |
|---|---|---|
| Piltdown 1. jaw | 0.03 | 3.9 |
| molar | 0.04 | 4.3 |
| canine | 0.03 | 5.1 |

GROUP 2

Upper Pleistocene Bone. More fluorine. Less organic matter.

| | % fluorine | % nitrogen |
|---|---|---|
| Piltdown 1. skull | 0.1 | 1.4 |
| Upper Pleistocene bone | 0.1 | 0.7 |

PILTDOWN 2

| | % fluorine | % nitrogen | |
|---|---|---|---|
| frontal fragment | 0.1 | 1.1 | Upper Pleistocene, probably from Piltdown 1. |
| molar tooth | 0.01 | 4.2 | Modern, probably from faked jaw, Piltdown 1. |
| occipital fragment | 0.03 | 0.6 | Modern, Piltdown 2 is from 3 sources! |

The teeth have been artificially planed down. Under a binocular microscope fine scratches from the abrasive show on both the canine and the molars. The occlusal surfaces are unnaturally flat with sharp borders. Although "wear" has exposed the dentine (especially in the canine), x-ray photographs show that no secondary dentine has been formed, as would have been the case if the "wear" had occurred during the animal's lifetime.

The jaw and the occipital fragment of Piltdown 2 were artificially stained to match the pieces of the skull. In the skull fragments there is a deep iron stain throughout their thickness, but in the jaw the stain is quite superficial. The extreme whiteness of the dentine and light color of the bone obtained by drilling deeply in the jaw shows that it was not naturally associated with the skull. In addition most of the cranial fragments and the jaw were chromate stained. At least part of the chromate staining was done by Mr. Dawson and this was known to Smith Woodward. The history of the chromate staining is too complicated to summarize. The least that can be said is that Mr. Dawson did it and that it made the jaw and skull look more alike.

The specimens were reanalyzed for fluorine content, using improved techniques and larger amounts of bone and dentine. The results are very different from those reported by Oakley and Hoskins (1950). (For example, the fluorine content of the molar of Piltdown 2 was given as 0.4% in 1950 and now appears as greater than 0.01). The remains are now shown not to be similar in fluorine content but to fall into two sharply defined groups. The skull fragments of Piltdown 1 and the frontal of 2, the pieces naturally and deeply stained with iron, have a fluorine content of 0.1% (the *minimum* for local Pleistocene bone). The jaw, teeth, and occipital of Piltdown 2 show no evidence of fluorination in the ground at all. If these bones are recent, they should contain organic material, and this was demonstrated by testing for nitrogen. In Table I the remains are put in two groups on the basis of fluorine and nitrogen content, and Piltdown 2 is considered separately.

The table requires little comment, except to point out that the organic content of the skull fragments is so high that it suggests a late Pleistocene dating. Neolithic bones were found to contain 1.9% nitrogen, that is little more than the skull. The authors promise a full presentation in a later number of the Bulletin, and that may include more comparative data on nitrogen content of bone of late Pleistocene age.

The authors conclude "that the elimination of the Piltdown jaw and teeth from any further consideration clarifies very considerably the problem of human evolution." It should be added that the elimination of *all* the Piltdown remains will result in even greater clarification. The authors speak of part of the skull being found in situ by Smith Woodward. But the jaw was also found "in situ" when Smith Woodward was present. What possible meaning does "in situ" have if pieces of bone are being faked and buried? Although much of the controversy over Piltdown has centered on the association of the jaw and skull, the great importance of the find was that up until 1950 many scientists believed that it was the earliest representative of the Homo sapiens line of evolution. Even when assigned to the third inter-glacial, it was far earlier than well-preserved and well-documented *sapiens* fossils. If the cranial fragments are merely those of an Upper Paleolithic man of the late Upper Pleistocene, they are of no importance. The removal of Piltdown greatly strengthens the general theory of human evolution outlined by Weidenreich (1946) in which Homo sapiens appears very late.

The solution of the Piltdown problem may mark the end of an era in the study of human evolution. Present thinking about human evolution follows channels which were largely determined when there were few fossils, and few techniques. Now there are enough fossils so that any major theory of human evolution should not be based on specimens which do not include at least part of the brain-case, some face, and jaw. This modest requirement would have prevented the vast waste of effort which resulted from the Piltdown fake. The greatest lesson of Piltdown for the student of human evolution is that *there never was enough of the fossil to justify the theories built around it.* If the history of man is written, using only those specimens where some

jaw, face, and brain case are preserved, specimens where faking is impossible and wholesale reconstruction unnecessary, then Homo sapiens appeared very late, perhaps within the range of carbon 14 dating. Neither the back of Swanscombe nor the top of Fontechevade permit the reconstruction of the face.

The elimination of Piltdown clarifies the evolution of the jaw, which now follows a consistent pattern from the australopithecines to ourselves. It clarifies the origin of Homo sapiens, and emphasizes the importance of basing theories on adequately preserved and dated fossils. Above all it stresses the importance of *studying the originals* with all the technique available. The nitrogen analysis alone is more conclusive than all the opinions of the experts who studied the Piltdown fake. It was the testing for fluorine which started the chain of events which led to the solution of the Piltdown problem. Microscope, x-ray, and chemistry have ended 40 years of debate.

## LITERATURE CITED


Boule, M. and H. V. Vallois

1952. *Les Hommes Fossiles.* Masson & Co. Paris.

Miller G. S.

1915. The Jaw of the Piltdown Man. *Smithsonian Miscellaneous Collections,* Vol. 65, pp. 1-31.

Oakley, K. P. and C. R. Hoskins

1950. New Evidence on the Antiquity of Piltdown Man. *Nature,* Vol. 165, pp. 379-388.

Weidenreich, F.

1946. *Apes, Giants, and Man.* The University of Chicago Press, Chicago.

Weiner, J. S., K. P. Oakley and W. E. Le Gros Clark

1953. The Solution of the Piltdown Problem. *Bulletin of The British Museum (Natural History) Geology,* Vol. 2, No. 3, pp. 141-146.


S. L. Washburn
University of Chicago
Chicago, Illinois

## CORRECTION!

In the Check List of Regional Publications (*American Antiquity,* Vol. 19, No. 2, p. 209) Ed Jelks' address is given as Houston, Texas!! Please change to read: Department of Anthropology, University of Texas, Austin 12.

## WENDELL C. BENNETT MEMORIAL

On September 6, 1953 American archaeology and anthropology and the social sciences in general lost one of their most distinguished members and effective spokesmen, Wendell Clark Bennett. Dr. Bennett was justly famous for his outstanding studies in South American archaeology, for his curatorial work at the Bishop Museum and the American Museum of Natural History, and as a teacher at Wisconsin and Yale. He was a former president of the American Anthropological Association. His contributions to the activities of the Social Science Research Council, the National Research Council, and similar bodies will prove of lasting value to his colleagues and to future generations of anthropologists.

In dedicating a memorial to Wendell Bennett it was believed that a fitting tribute to the man from his many friends, would be the establishment of a fund to aid in the future education of his daughter, Martha, now age 12. On behalf of the Wendell C. Bennett Memorial Fund Committee, I am writing to you to contribute if you would care to do so.

Checks should be made payable to the *Bennett Memorial Fund, American Anthropological Association* and sent to Mr. *Frederick Johnson, Box 71, Andover, Mass.* Although this is not an official business of the American Anthropological Association, Mr. Johnson, the Executive Secretary, has made the association offices available to the committee for this express purpose.

GORDON R. WILLEY
For the Committee

## MEMOIRS OF THE SOCIETY FOR AMERICAN ARCHAEOLOGY

Numbers 1-5, 8 and 9, are bound and sold separately.

1. *Big Bead Mesa: an Archaeological Study of Navaho Acculturation, 1745-1812.* By DOROTHY LOUISE KEUR. vi+90 pages, 4 plates, 5 figures. $1.00.
2. *The Tchefuncte Culture, an Early Occupation of the Lower Mississippi Valley.* By JAMES A. FORD and GEORGE I. QUIMBY, JR., with an Appendix on the Skeletal Remains by CHARLES E. SNOW. xii+113 pages, 12 plates, 23 figures, 3 folding tables. $1.50.
3. *The Prehistory of Northern North America as Seen from the Yukon.* By FREDERICA DE LAGUNA. x+360 pages, 30 plates, 33 figures. Out of print.
4. *A Reappraisal of Peruvian Archaeology.* Assembled by WENDELL C. BENNETT. x+128 pages, 72 figures, 6 tables. $2.00.
5. *The George C. Davis Site, Cherokee County, Texas.* By H. PERRY NEWELL and ALEX D. KRIEGER, with an Appendix on the Corn by VOLNEY JONES. xvi+255 pages, 66 figures, 20 tables. $3.50.

Numbers 6-7, bound under one cover, are sold only as a unit. $2.50.

6. *Archaeology of the Columbia-Fraser Region.* By MARIAN W. SMITH. viii+46 pages, 4 figures, 3 tables.
7. *Cattle Point: a Stratified Site in the Southern Northwest Coast Region.* By ARDEN R. KING. xii+94 pages, 25 figures, 13 tables.
8. *Radiocarbon Dating.* Assembled by FREDERICK JOHNSON. vi+65 pages, 2 figures, 2 tables. $1.50
9. *Asia and North America: Transpacific Contacts.* Assembled by MARIAN W. SMITH. vi+97 pages, 27 figures. $1.50.

### BACK ISSUE PRICES

Money should accompany the back issue orders. Volumes I through VIII cost $3.00 per volume or $1.00 per single number. Volumes IX through XII cost $4.00 per volume or $1.25 per single number. Volumes XIII on cost $6.00 per volume or $1.75 per single number. *Out of Print*: Vol. 4, Nos. 1, 2; Vol. 5, Nos. 1, 2, 3, 4; Vol. 6, Nos. 3, 4; Vol. 11, No. 1; Vol. 12, Nos. 1, 2, 3; Vol. 13, No. 1.

## INFORMATION FOR AUTHORS

*American Antiquity* publishes original papers in the field of American archaeology, as well as comments, reviews, and discussions on matters pertaining to this field. Articles on other regions, or on such related subjects as geography, history, and ethnography, may be accepted if of special significance to American archaeology.

Accepted papers are published in the order of their receipt, and when printed all papers bear the date of submission.

Manuscripts must be typewritten on standard 8½ × 11 inch paper, double spaced, and on one side of the page only. Footnotes are used *only* when essential information cannot be incorporated into the text, and should be on separate sheets, also double spaced and on one side only. Bibliographies must follow a standard form. Citations from journals and series should be given as follows:

JENKS, ALBERT E., AND LLOYD A. WILFORD
1938. The Sauk Valley Skeleton. *Bulletin of the Texas Archaeological and Paleontological Society*, Vol. 10, pp. 136-68. Abilene.

Citations from separate books or pamphlets:

JENKS, ALBERT E.
1936. *Pleistocene Man in Minnesota.* Minneapolis: University of Minnesota Press.

When illustrations and/or tables cost more than the price for one full page of halftones, the author must pay for the excess. Photographs for reproduction should be on a glossy paper, unmounted, and of a proportion suitable for reduction to 5¾ × 8⅜ inches or a portion thereof, making due allowance for captions. Line drawings should be in India ink on smooth, hard-surfaced paper, with figures written lightly in pencil on the margin or the back.

Galley proof, only, will be sent to authors, who will be charged for excessive correction costs. Upon publication, manuscripts, illustrations, and engravings will be destroyed unless their return has been specifically requested.

Reprints without covers are supplied at the following minimum prices, and must be ordered when corrected galley proofs are returned.

| *Copies* | *1–4 pp.* | *5–8 pp.* | *9–12 pp.* | *13–16 pp.* | *17–20 pp.* | *21–24 pp.* | *25–28 pp.* | *29–32 pp.* |
|---|---|---|---|---|---|---|---|---|
| 50 or less | $7.25 | $10.20 | $11.85 | $14.30 | $17.05 | $19.80 | $22.55 | $25.85 |
| 100 | 9.70 | 11.90 | 15.60 | 18.50 | 21.12 | 25.10 | 27.06 | 33.00 |
| Extra 100's | 2.20 | 3.85 | 5.50 | 7.70 | 9.35 | 11.00 | 13.20 | 14.30 |

Covers with title and date of issue of *American Antiquity* will be furnished at the following rates: 25 or less for $7.25; additional covers 4 cents each.

# TABLE OF CONTENTS

## ARTICLES


| | | Page |
|---|---|---|
| On the Use of the Concept of Area Co-Tradition | Irving Rouse | 221 |
| Suicide, Sacrifice and Mutilations in Burials at Venado Beach, Panama | S. K. Lothrop | 226 |
| The Pointed Mountain Site near Fort Laird, Northwest Territories, Canada | Richard S. MacNeish | 234 |
| Observations on the Butchering Technique of some Aboriginal Peoples, Nos. 3, 4, 5 and 6 | Theodore E. White | 254 |


## NECROLOGY


| | |
|---|---|
| Wendell C. Bennett | 265 |


## FACTS AND COMMENTS


| | | |
|---|---|---|
| A Note on Fluted Point Relationships | John Witthoft | 271 |
| A Comment on "Fluted Point Relationships" by John Witthoft | Alex D. Krieger | 273 |
| Plaster Under the Newport Tower | Frederick J. Pohl | 275 |
| Answer to "Plaster Under the Tower" | William S. Godfrey | 277 |
| A Carved Atlatl from Northwest Coast | Herbert C. Taylor, Jr. and Warren Caldwell | 279 |
| Notes on Third Interglacial Artifacts | William B. Roosa and Stewart L. Peckham | 280 |
| Artistic Connections between the Chichen Itza Toltec and the Classic Maya | Robert L. Rands | 281 |
| Additional Notes on the Poverty Point Site in Northern Louisiana | James A. Ford | 282 |
| Site on Fremont Island in Great Salt Lake | Jack R Rudy and Earl Stoddard | 285 |
| Recent Publications on American Indian Physical Anthropology | Erik K. Reed | 290 |


## BOOK REVIEWS


| | | |
|---|---|---|
| Taylor: A Study of Archeology | Woodbury | 292 |
| Ford: Measurement of Prehistoric Design Elements | Rouse | 296 |
| Ferdon: Tonala, Mexico. A Survey | Ekholm | 297 |
| Martin, et al: Mogollon Cultural Continuity and Change | Kidder | 298 |
| Orr: Excavations of Pershing County Caves | Malcolm F. Farmer | 300 |
| Kroeber (editor): Anthropology Today and Tax, Eiseley, Rouse, Voegelin: Appraisal of Anthropology Today | Griffin | 301 |
| Bowers: Mandan Social and Ceremonial Organization | Cooper | 302 |
| Notes and News | | 304 |
| Wendell C. Bennett Memorial | | 316 |



AMERICAN ANTIQUITY IS INDEXED IN THE INTERNATIONAL INDEX

UNIVERSITY OF UTAH PRESS, SALT LAKE CITY, UTAH